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Edited by

R. H. RASTALL, M.A., Sc.D., M.Inst.M.M.,

FELLOW OF CHRIST'S COLLEGE AND
UNIVERSITY LECTURER IN MINERALOGY AND PETROLOGY, CAMBRIDGE.

assisted by

Professor W. S. Boulton, D.Sc.
Sir T. H. Holland, K.C.S.I., D.Sc., F.R.S.
Professor J. E. Marr, Sc.D., F.R.S.
Professor W. W. Watts, Sc.D., LL.D., M.Sc., F.R.S.
Henry Woods, M.A., F.R.S.
Sir Arthur Smith Woodward, LL.D., F.R.S.

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ORIGINAL ARTICLES.

Some Recent Contributions to the Pleistocene Succession in England.

By K. S. Sandford.

IN the autumn of 1929 certain papers on the Pleistocene geology of the Midlands were published by authors mutually unaware of each other's results. These include :—

Miss M. E. Tomlinson, "The Drifts of the Stour-Evenlode Watershed and their Extension into the Valleys of the Warwickshire Stour and Upper Evenlode," *Proc. Birmingham Nat. Hist. and Phil. Soc.*, xv, 8, 1929, 157–96. (Read 7th December, 1928.) A. Jowett and J. K. Charlesworth, "The Glacial Geology of the Derbyshire Dome and the Western Slopes of the Southern Pennines," *Quart. Jour. Geol. Soc.*, lxxxv, 3, 1929, 307–34. (Read 23rd January, 1929.) And "The Erratic Rocks and the Age of the Southern Limit of Glaciation in the Oxford District," which I read before the Geological Society on 5th December, 1928, published at the same time as Messrs. Jowett and Charlesworth's paper (*Quart. Jour. Geol. Soc.*, 1929, 359–88).

Miss Tomlinson and I knew of each other's work and general conclusions, but neither of us was aware that the Geological Survey was investigating the adjacent area until Mr. H. G. Dines' report appeared in the *Summary of Progress of the Geological Survey*, pt. ii (pp. 66–71), for 1927 (published late in 1928), too late for either of us to refer to it in any detail.

Messrs. Jowett and Charlesworth make no reference to an important earlier paper by Miss Tomlinson (*Quart. Jour. Geol. Soc.*, lxxxi, 1925, 137–69) which, with Dr. L. J. Wills' well-known paper on "The Development of the Severn Valley in the neighbourhood of Ironbridge and Bridgenorth" (*Quart. Jour. Geol. Soc.*, lxxx, 1924, 274–314), puts our knowledge of the Severn basin and of its history on a new footing.

In February, 1931, Dr. A. W. Groves' paper on " The Unroofing of the Dartmoor Granite and the Distribution of its Detritus in the Sediments of Southern England " appeared (*Quart. Jour. Geol. Soc.*, lxxxvii, 1931, 62–96), though it had been read before any of the others were published (March, 1929). Both he and I were abroad at the time, with extremely slow means of communication, and therefore could achieve little. In other circumstances profitable collaboration might have been possible on the Upper Thames erratic rocks derived (at some period) from south-westerly sources.

In the summer of 1930, a joint discussion of the British Association at Bristol provided a wide range of opinions on Pleistocene climates, most of them disadvantageous to the application of the Alpine glacial sequence to this country, a welcome sign of independence. Some, but not all, of the contributions appeared in the *Report.*

Another crop of notes and papers followed in the autumn of 1930, most of which have now found their way into print. They include an article by Mr. M. C. Burkitt in *Nature* (4th October), in which he pays Mr. Reid Moir and me the compliment of using as standards of reference the successions which we have suggested in East Anglia and in the Upper Thames respectively. Mr. Henry Dewey read a paper before the Geological Society on " The Palaeolithic Deposits of the Lower Thames Valley " (3rd December) and made " Palaeolithic Thames Deposits " the title of his Presidential Address to the Prehistoric Society of East Anglia (published in the *Proceedings* for 1930, vol. vi, pt. iii) : in both attention is concentrated on the Middle and Lower Thames and the upper part of the river basin is not discussed.

In the same volume as Mr. Dewey's Presidential Address will be found an important paper by Mr. F. W. Shotton on Palaeolithic implements found near Coventry, and I for one am grateful to him for dispersing a cloud of ignorance of the central Midland Pleistocene beds. He adds a correlation table which compares his succession with Miss Tomlinson's and with mine. Unfortunately he does not seem to have seen the two papers referred to at the beginning of these notes. He also attempts correlation with East Anglian successions.

We are now grateful beyond measure to Professor P. G. H. Boswell for his inspiring summary of the Glacial Deposits of East Anglia (*Proc. Geol. Assoc.*, xlii, 2, 1931, 87–111). In this he combines his own former and more recent researches with the enormous body of publications by other authors into a form readily understandable by those who, not having spent their lives in East Anglia, have struggled to follow the confused literature on the subject. This short paper will remain a standard work for many years, and we should like a second edition of Mr. Reid Moir's book (*The Antiquity of Man in East Anglia*, Cambridge University Press, 1927) to keep it company. At the conclusion of his paper, Professor Boswell devotes a section to brief comments on recently-published studies of British glacial deposits and the Upper Thames is once more referred to, certain correlations being suggested. Difficulties are also pointed out, one of which we can dispel at once.

Professor Boswell is under the impression that, in the paper already mentioned, I regarded the oldest glacial deposit in this district, called the Plateau Drift, as older than the Cromer Forest Bed, a correlation which, in view of his paper, will not stand. I tried, rather, to convey the impression that the question was one that could not be answered with any reasonable certainty in the existing state of knowledge. Beyond pointing out certain possibilities and difficulties, I intended to leave the question open, and only in the discussion (p. 388) did I express a personal belief that I considered the glaciation in question " antedated any other Pleistocene glaciation in the British Isles, . . . the relation to the period of the Cromer Forest Bed remained in doubt, although (I was) inclined to place the glaciation immediately after that time ". It goes without saying that detailed correlations of successions with those of East Anglia (which I did not attempt) prior to Professor Boswell's paper are now out of date, but it seems that the quotation given here was remarkably close to the truth, and it should serve to remove any misapprehension.

The other difficulty which is mentioned is the need to find in the Oxford district the deposits corresponding to Professor Boswell's Chalky-Jurassic Boulder Clay (= Chalky-Kimmeridgian and Chalky-Oxfordian Boulder Clay of other authors, and Chalky Boulder Clay of Cambridgeshire). This has now been identified by Miss Tomlinson.

It seems a suitable moment to draw together some of the papers mentioned earlier in these notes.

EAST ANGLIAN SUCCESSION.

The following are some of the salient points of Professor Boswell's paper, and of observations arising therefrom :—

(1) The so-called Weybourne Crag glaciation and Cromer Forest Bed warm period are myths. The British climate and sea temperature were becoming steadily colder from Middle Pliocene times onwards. The ecological bearings of these changes are of the greatest interest, and are vividly summarized in the paper.

The scantily preserved *Leda myalis* Beds and Arctic Freshwater Beds seem to mark the final stages of this degeneration, and the latter is followed by the arrival of heavy ice. Drifting sea ice and bergs visited the northward-advancing East Anglian coast from more northern latitudes as early as Red Crag times, and periodically thereafter, but an iceberg does not—or should not—make an ice age.

(2) The first part of the Pleistocene succession thus seems to be Cromer Forest Bed → *Leda myalis* → Arctic Freshwater Beds, *or*

Cromer Forest Bed → Westleton gravel beds (in parts of Norfolk). In both cases a boulder clay follows, in East Norfolk and Suffolk called the Norwich Brickearth. This in turn is followed by "Middle Glacial" sands and gravels, overlain by the widely preserved Chalky-Jurassic Boulder Clay.

(3) Suspicion is attached to the Cromer Till and Contorted Drift, which locally overlie the Cromer Forest Bed—Arctic Freshwater Bed series mentioned above. Research is still in progress, but there seems sufficient reason to suppose that these deposits are the local equivalent of the Chalky-Jurassic Boulder Clay. This is a marked change from older beliefs.

(4) The succession is continued in the famous Hoxne sections, which have been thoroughly reinvestigated in the last few years, with the vindication of the original observations.

The bottom of the Hoxne basin is indubitably formed by the Chalky-Jurassic Boulder Clay. There follow Brickearths, with Lower Acheulean implements, a temperate phase; an interpolation of more arctic conditions is followed by more temperate conditions and Upper Acheulean, and then Mousterian cultures. The record is brought to an abrupt end by the arrival of true glacial conditions once more, represented by the Upper Chalky Boulder Clay. Through this phase, however, Mousterian man survived in more favoured regions, and Late Mousterian implements are found in overlying deposits notably in the Ipswich district (where, moreover, the complete Hoxne succession has been verified in the Foxhall Road (Derby Road) exposures). The distinguished part played by Mr. Reid Moir in disentangling many of these problems, especially from the archaeological side, is too well known to need comment here.

(5) It will be noticed that three well-marked boulder clays are so far identified: Norwich Brickearth, Chalky-Jurassic, and Upper Chalky Boulder Clays. The probability that they may be correlated with the Basement, Purple, and Hessle Boulder Clays respectively of the Yorkshire coast is noted.

(6) A fourth boulder clay, known as the Brown or Hunstanton Boulder Clay, has recently been shown by Mr. Reid Moir to contain Upper Palaeolithic implements. Professor Boswell recalls the fact that the floors of the Upper Palaeolithic caves of North Wales are sealed with a local boulder clay, a fact which has long caused some perplexity, but which now falls into line with this confirmatory evidence of some recrudescence of (probably localized) glaciation.

(7) It will be noted that no mention has yet been made of the Chellean. It represents one of the outstanding problems, and more information would have been welcome had it been available. On all counts, however, the Chellean seems to be separated from the Lower Acheulean by a period of glaciation, i.e. the Chalky-Jurassic Boulder Clay, and rolled Early Chellean implements are found below that horizon, in the Middle Glacial Sands and Gravels. Of

the rest of the Chellean industry little more seems to be forthcoming in East Anglia. There remains the supposed Early Chellean of the Cromer Forest Bed " floors " found by Mr. Reid Moir. The question remains unsettled as to whether Chellean Man straddled an ice age—the Norwich Brickearth—or properly belongs to the interglacial phase between it and the Chalky-Jurassic Boulder Clay. If the latter is true, a new name is required for the Cromer Forest Bed industry. On the whole perhaps this is the best solution, but it is clear that much yet remains to be found out in this part of the sequence, from which probably large sections have totally disappeared, at least in East Anglia, under the oft-repeated advances and retreats of the ice. If gaps occur here they may be filled elsewhere.

The following is a summary of Professor Boswell's East Anglian succession, based on the tables and text of his paper :—

(*k*) Present Day.
(*j*) River alluvium, Temperate, Upper Magdalenian, Tardenoisian, Neolithic.
(*i*) Hunstanton or Brown B.C. *Glacial.* Derived U. Aurignacian, and ? L. Magdalenian.
(*h*) Hill washes, ancient alluvium. ? Cold to temperate. { Solutrian. / Aurignacian.
(*g*) Hill washes, river silts, and brickearth, etc. Temperate to ? cold. U. Mousterian.
(*f*) U. Chalky B.C. *Glacial.* Derived Mousterian and Acheulian.
(*e*) Brickearths and gravels, etc. Temperate with a cold oscillation :— Lower Mousterian, U. Acheulian, cold oscillation, L. Acheulian.
(*d*) Chalky-Jurassic B.C. and ? Cromer Till. *Glacial.* ? Land surface—Chellian.
(*c*) " Middle Glacial " Sands and Gravels. ? Milder. ? Chellian.
(*b*) Norwich brickearth. *Glacial.*
(*a*) [1] Increasing cold through Pliocene, *Leda myalis*, and Arctic Freshwater Bed.

UPPER THAMES SUCCESSION: THE STOUR-EVENLODE WATERSHED: EAST ANGLIA.

We will examine this in the light of what has been said in the foregoing pages.

The oldest deposits are the Plateau Drifts, erratic material certainly introduced by some glacial phase, by far the greatest in this district, and subsequently redeposited in a series of terraces which have been demonstrated by the Rev. Charles Overy. The erratics are drawn from widely distant parts of the United Kingdom, and Scandinavian rocks have recently been added to the list.

At 140 feet above present river-level, Mr. Overy has found primitive Chellean implements near Reading. At 100 feet in the Oxford area is the well-marked Hanborough terrace, containing a " warm " fauna of marked early Pleistocene type. The only implements which can be attributed to this terrace are rolled Chellean; the evidence is scanty around Oxford, but improves nearer Reading. What is more important within the district about Oxford is the total absence of any Acheulean implements at this horizon, in contradistinction to the Lower Thames.

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[1] The letters (*a*) to (*k*) are used for all tables in these notes to indicate contemporaneity of beds bearing the same letter. Other authors' spellings of terms, e.g. Chellian, Acheulian, Kimmeridgic, are adopted when directly quoted.

The Upper Thames and its tributaries became remarkably mature at the 100 foot level, and they were meandering in their alluvial tracts when renewed downcutting carried them, and their meanders, some 50 feet through hard limestones and clays alike. The meanders remain in use at the present day, with 100 foot cliffs and steep slopes.

At the new (50 foot) level another terrace was deposited, called in this district the Wolvercote Terrace. It is evident that some remarkable processes had been at work to cause such sudden and drastic movements. The new terrace has been reduced to small patches by subsequent erosion. It contains only rolled, rather early, Chellean implements, which are rare. No Acheulean implements have been found. It is barren of life. Not a single bone or tooth has been proved satisfactorily to have come from it.

How far do the successions of East Anglia and of the Upper Thames agree up to this point?

We may accept Professor Boswell's statement that Scandinavian erratics had not arrived in this country before Cromer Forest Bed times, and take advantage of their recent discovery in the Upper Thames to correlate, as Professor Boswell suggests, the local Plateau Drift and the Norwich Brickearth.

We may remark that the evidence in the Oxford district points unmistakably to an enormous period of time elapsing between this glacial episode and the construction of the 140–100 foot Chellean terraces, of which the Middle Glacial Sands and Gravels of East Anglia seem to be the counterpart, together with the probable gap in the East Anglian sequence, occupied by a period of land surface and Chellean industry.

It remains to find the local equivalent of the Chalky-Jurassic Boulder Clay.

Looking first to the north-east of the Oxford district, I have referred in earlier publications to a remarkably "young" boulder clay—judged by comparison with the local Plateau Drift which is totally decalcified (save for rare cherty limestone erratics)—in which Scandinavian rocks seem to be fairly abundant. It abounds in fresh flint, chalk, and material derived from Oxfordian and other Jurassic clays, as well as in northern British rocks which occur in the "North Sea Drift". It ascends the Ouse valley to the Thames-Ouse watershed, but I have no evidence that it crosses it between Buckingham and Banbury, although I believe it enters the Thame basin in the Aylesbury district. Without looking further down the Ouse valley than Biddenham, there seems no reason to doubt that this is the pre-Acheulean Chalky-Jurassic Boulder Clay. It remains to be investigated in detail.

Looking to the north-west, up the Evenlode valley, we have been aware that another boulder clay, distinct from the Plateau Drift, was to be seen on the valley floor, and it has been considered probable that it was analogous to that just mentioned in the Ouse valley. From Oxford we have been unable to give it the desired attention, and we welcomed the news that Miss Tomlinson was approaching it from the other side of the watershed, i.e. from the Severn basin up the Stour valley.

Her recent paper has been quoted at the beginning of these notes, and it is only fair to her to say that it has not received a tithe of the attention it deserves. A number of valuable papers by Mr. L. Richardson on this Evenlode-Stour watershed are also incorporated in her treatise, and need not be referred to separately here.

Miss Tomlinson recognizes on the Stour side of the divide :—

1. Plateau Drift analogous to that of the Oxford District and occurring both on the hill-tops and, to a more marked extent than about Oxford, down the valley sides. Ice was clearly the vehicle which introduced it, but difficulty arises, as in Oxfordshire, in discovering more precisely how it introduced it. The drift is recognized to be separated by a very long interval from the next deposit :—

2. The Campden Tunnel Drift. Identified as an undoubted product of land ice, mainly of north-westerly origin with perhaps some north-easterly sources in Leicestershire. Welsh rocks occur, and dolerite. This drift is separated from another undoubted product of land ice, the Moreton Drift, by some fluviatile gravels called the Ditchford Gravels.

3. The Moreton Drift. One of its chief characters is the abundance of fresh flint. Leicestershire syenite and other rocks occur, and a north-easterly origin of the land ice which transported it is evident. Miss Tomlinson was unable to confirm the remarkable erratics, reported by earlier authors, which on field evidence may be ascribed to this drift. These include striated lumps of chalk, red chalk, Cumberland and Scottish rocks.

Miss Tomlinson is very explicit that the interval between the Campden and Moreton Drifts (represented by the Ditchford beds) " need not have been of long duration nor is there any reason to believe it assumed the magnitude of an Interglacial Period. . . . There is evidence that (the two drifts) were to some extent contemporaneous . . . (and) it is therefore very probable that Campden Tunnel Drift, Ditchford Gravels, and Moreton Drift are all products of the same cold period " (p. 185).

Referring to the Oxford district, after sifting the evidence very carefully, Miss Tomlinson concludes that " the Wolvercote Terrace gravels are really a down-valley extension of Moreton Drift, and were laid down by melt-water from the ice which deposited the Moreton Drift in the Moreton Gap " (p. 188). In this from my own knowledge of the facts, I would heartily support her.

She makes assurance double sure by showing that the Moreton Drift is *not* the contemporary deposit of the much later (Mousterian) disturbed beds of the Oxford district.

Thus Professor Boswell's requirement of the Oxford district seems to be amply satisfied, and an earlier statement of mine that " at no time subsequent to the introduction of the Plateau Drift did heavy ice enter the district" seems to be confirmed.

To recapitulate, the first part of the sequence now seems to be established :—

3. Chalky-Jurassic B.C. = Moreton Drift = Wolvercote Terrace.
2. Middle Glacial Sands = 140–100 ft. Terraces of Upper Thames = Chellean (in part).
1. Norwich Drift = Plateau Drift.

We may now continue the series.

After the Wolvercote Terrace was deposited renewed erosion in the Upper Thames Basin lowered the rivers some 30 feet, and the centres of the channels to present river-level. Gravels were deposited which abound with remains of mammoth, and of woolly rhinoceros, while bison, horse, and other animals occur. (The full list will be found most conveniently in the second edition of *The Geology of the Country around Oxford* (*Mem. Geol. Survey*, 1926) or in papers which I contributed to the Geological Society (*Quart. Jour. Geol. Soc.*, lxxx, 1924, and lxxxi, 1925)).

Musk ox and reindeer are absent.

An unknown thickness of gravel was deposited, but was eroded again to its present surviving thickness of 0 to 10 feet. A marked gap must thus exist in the series between this and the overlying gravels which are of similar appearance. The junction of the two gravels is sometimes marked by an eroded surface, but it is often difficult to trace. In any given exposure one or other may be missing, but the two have repeatedly been seen in the same section, each with its own fauna. The upper gravel is rich in remains of hippopotamus, *Elephas antiquus*, red deer, horse. *Cervus megaceros* (Irish Elk) occurs for the first time, with other mammals. *Corbicula fluminalis* and other shells of temperate habitat are numerous. The lower gravels have provided no mollusca. A maximum of about 10 feet of this gravel survives.

In the double terrace thus described, termed the Summertown-Radley Terrace, Lower, Middle, and Upper Acheulean implements are concentrated. Little can be gauged from the state of erosion or freshness in such a deposit. This is the first appearance of Acheulean implements in the series. It has not been possible to assign them with absolute certainty some to the lower, some to the upper gravels on field evidence. The cold fauna was probably surviving from the previous glacial phase of the Moreton Drift; it left the area before the upper gravels were laid down. A warm fauna comparable with that which replaced it has not been fully accounted for, so far as I am aware, in Norfolk and Suffolk, but has been found in the gravels around Cambridge. It has been identified in a corresponding series of deposits of the Warwickshire Avon by Miss Tomlinson (q.v.).

The sequence related above looks very much like the beginning of the Hoxne series, to which I will return later.

A remarkable aggradation of the rivers of the Upper Thames basin continued the warm phase already described, and the valleys seem to have been filled, at least in the Oxford plain, to the height of the Wolvercote (50 foot) Terrace, in which hollows were now filled (e.g. Wolvercote Channel). Its floor is lined with gravels in which are the surviving members of the warm fauna, especially *Elephas antiquus* and a large red deer, with the highest type of Upper Acheulean implement and small tools assigned to the Micoque industry.

In the overlying sands the climate is seen to change, culminating in thin peats with temperate and sub-arctic plants and mosses followed by finely bedded sandy clays, in which reindeer appears for the first time with a few supposed Mousterian flakes of poor workmanship.

The upper beds of clay are disturbed and plicated and are covered by " warp ", a decalcified sandy clay without bedding, plicated at base, with balls of sand thrust into the underlying clay. There is nothing to suggest that this is a boulder clay: there is every feature to show that it resulted from the washing out of fine clay particles and from movement through annual melting and freezing of the upper few feet of ground ice.

The whole sequence of the Channel indicates refrigeration of climate which might precede a glacial epoch. There can remain no doubt that this was the phase of the Upper Chalky Boulder Clay of East Anglia. The correlation with a section such as that at Hoxne or Foxhall Road (Derby Road), Ipswich, is fairly complete. It will be noticed that a short rather than a " long chronology " such as Mr. Burkitt suggested in *Nature* is indicated. Mr. Burkitt considered the Hoxne section to cover the period from the Plateau Drift to the top of the Wolvercote series. It should be pointed out that the " warp " is general over the Oxford district, in one form or another, and that it is not limited to the one locality.

A probable correlation of the Upper Thames Basin with the East Anglian sequence, incorporating the evidence of the Evenlode-Stour watershed just discussed, seems to be as shown in the table on page 10.

Little remains to be said of the rest of the series. The remainder of the history is occupied in the Upper Thames by the removal of most of the gravel previously deposited, with a pause of some duration at the present level of the top of the Summertown-Radley Terrace. It was this pause which determined its height. The festooning of its surface through presence of frozen soil was widespread, these signs of a still rigorous climate being covered locally by a foot or so of river gravel, probably during floods due to spring melting. Erosion was then continued and a deep channel cut, now filled with gravel. Low banks and islands of Flood Plain gravel were also formed.

As yet we have no detailed knowledge of the human and faunal contents of these gravels, beyond the occurrence of well-preserved teeth of mammoth at the bottom of the deep channel, probably not derived specimens. The festoons of the gravel just mentioned probably correspond with the Upper Palaeolithic hill washes of East Anglia—it can hardly be otherwise.

UPPER THAMES.	EAST ANGLIA.
(*k*) Present day.	
(*j*) River alluvium. Temperate.	River alluvium: Neolithic. Tardenoisian. U. Magdalenian.
(*i*) Mammoth at base of deep channel: channel filled. Flood Plain gravels.	Hunstanton Boulder Clay: ? L. Magdalenian Derived Aurignacian. *Glacial.*
(*h*) Continued downcutting, excavation of deep channel.	Hill washes, ancient alluvium: Solutrean. Aurignacian.
(*g*) Warp, hill washes, and downcutting of river bed, with pause.	Hill washes, river silts, and brickearth. U. Mousterian.
(*f*) "Warp" non-fluviatile, frozen soil—land surface. (Wolvercote Channel.)	Hoxne: Upper Chalky Boulder Clay. *Glacial.* Brickearth, Early Mousterian, temperate. Mammoth-Reindeer Gravels. U. Acheulian.
(*e*) River clays, Mousterian, Sand and peat, Cold temp. or subarctic. (Wolvercote Channel.) Lower Gravels, Warm, Late U. Acheulian-Micoque. (Wolvercote Channel.) Upper Summertown-Radley Terrace, Warm fauna. —Erosion— Lower Summertown-Radley Terrace, Cold fauna.	U–L Acheulian: Arctic brickearth. Peat, temperate sylvan. Prob. Lower Acheulean. Temperate brickearth (on boulder clay (*d*)). Prob. Lower Acheulean.
Interval of Erosion.	
(*d*) Moreton Drift, Wolvercote Terrace with derived Chellean.	Chalky-Jurassic Boulder Clay. *Glacial.*
(*c*) 140–100 ft. Terrace group Chellian.	"Middle Glacial" Gravel and ? Land surface. Chellian.
(*b*) Plateau Drift.	Norwich Drift. *Glacial.*
(*a*) No detailed record of Pliocene times preserved in Midlands. See East Anglia.	

No local equivalent of the Hunstanton Brown Boulder Clay is known. These records are buried below permanent water-level.

I do not propose to continue the correlation over all southern England and the Midlands. Miss Tomlinson, entirely conversant with the Upper Thames series, has shown in her paper how her Avon terraces represent an analogous history, and that a period of aggradation exists there also. Mr. Shotton has compared his series with Miss Tomlinson's, but I am not qualified to discuss how this will be affected by her more recent paper.

In the Cambridge district, the whole of the evidence is being overhauled once more, and new added to it, and we must await developments. I would remark only that the Acheulean-Mousterian aggradation noticed in the Upper Thames is recognized in an increasing number of river basins. Something of the same sort was emerging from the Rev. L. F. A. Edgell's work on the New Forest gravels, and it is to be hoped that he will publish his results on his return from Persia.

UPPER AND LOWER THAMES.

No discussion of the Thames can be complete if only a part of the river is considered. One has only to glance at Mr. Dewey's table to see how wide are the apparent discrepancies between Lower and Upper Thames, and it is clear that the two extremities of the river have had different histories, as might be expected. Mr. Dewey draws on his knowledge of the Middle Thames, and on work that has been published on it by many eminent authors, which need not be treated separately here.

The deposits of the Lower Thames seem to fall into certain groups :—

1. Boulder clay (Kimmeridgic Boulder Clay).
2. Swanscombe series, with base at 88 feet above sea-level, called the 100 foot or Boyn Hill Terrace.
3. Taplow or 50 foot terrace.
4. Crayford Brickearth series and Coombe Rock.
5. Flood Plain and lower gravels.

I will take these in order, but, for convenience, will refer to the second before the first.

2. *Swanscombe Series.*—About 30 feet of gravels divided by loams into Lower, Middle, and Upper beds. In the lower occurs the Clactonian flake industry, totally dissimilar in technique, but not far removed in time from the Acheulean, which occurs from early to late forms in ascending series through the overlying beds. The Chellean seems to take no part in the series, except that it is earlier, and specimens which occur are derived.

This has nothing in common with the 100 foot terrace ot the Upper Thames, where neither Clactonian nor Acheulean seem to occur. The faunas are "warm" in both places. In the Upper Thames the Acheulean is first seen in the double Summertown-Radley terrace.

1. *Boulder Clay.*—Kimmeridgic Boulder Clay is placed before the Swanscombe series in time. From what I know of it I do not think that this is the equivalent of the Plateau Drift. A boulder clay is fairly certainly older than the gravels of the 100 foot terrace in the Lower Thames, but which gravels ? The critical sections in Essex yielded neither bones nor implements.

It is improbable that a pre-Acheulean boulder clay interrupted any part of the Swanscombe series. It seems more probable that the 140–100 foot Chellean group of the Upper Thames is represented only by Dartford Heath gravels, which form a marked platform rather above, and not far away from, the Swanscombe beds, and that any other Chellean deposits at lower level have been removed and their place taken by the Swanscombe series.

If this is so, it seems probable that the whole of that series is post-Kimmeridgic Boulder Clay as stated, and that that deposit is the pre-Acheulean Chalky-Jurassic Boulder Clay of East Anglia. In that event the Dartford Heath gravels were preceded by the Norwich Brickearth–Plateau Drift of Oxford glaciation, a fact which would be in harmony with the nature of pebbles found in those gravels, and with other evidence.

This is equivalent to saying that the Swanscombe series are later than the Moreton Drift–Wolvercote Terrace epoch, and that there is no evidence to show that the river regained the 100 foot level in the Oxford district at any subsequent time.

Such a hypothesis is not as revolutionary as it appears at first sight. It amounts only to saying that an aggradation occurred in the Lower Thames as a result of excessive erosion in the Upper Thames. It is not mere chance that Acheulean implements are totally lacking in the Oxford district until we find them concentrated in the Summertown-Radley terrace just as they are concentrated at Swanscombe. The vertebrate faunas, excepting the assemblage in the lower gravels about Oxford, are of the same type, but the warm fauna of Oxford, especially the Proboscidea, seems to be of more recent age, of Middle rather than Lower Pleistocene affinities. The difficulty centres round the cold fauna of the lower gravels of Oxford, which is not represented at Swanscombe. Is it a fauna surviving from an immediately preceding Moreton Drift glaciation, or is it of somewhat later age, the equivalent of the cold phase in the middle of the Hoxne section? The first suggestion seems the more likely, but it lacks support at Swanscombe, and again the Proboscidea are of younger rather than older Pleistocene type, since the Siberian mammoth is abundant and fully evolved, living with survivors of an older mammoth stock. I think the second suggestion is the more probable, that the Summertown gravels, as a group, are not widely separated from, but younger than, the Swanscombe series, and that the post-Wolvercote Terrace interval was considerable.

At the end of the warm phase, in uppermost Acheulean times, the Upper Thames was back at the 50 foot level again. The Swanscombe series start at about 88 feet level, and mount to over 100 feet.

This does not deny that a 100 foot terrace such as we know in the Upper Thames existed in the rest of the river; undoubtedly it did, and probably the greater part of the Boyn Hill terrace of the Middle and Lower Thames belongs to it. The hypothesis simply takes into consideration that an aggradation in Acheulean times may have overlapped and locally replaced the lower part of the 140–100 foot gravels with Chellean (and no Acheulean) implements. In the absence of Clactonian flakes from the Upper Thames there is no direct evidence for including the Lower Gravels of Swanscombe in the discussion.

(3) *Taplow Terrace*, stated by various authors as the 30, 40, and 50 foot terrace.

A summary of the considerable literature on this terrace leaves one convinced that it is either a multiple terrace, like the Summertown-Radley terrace, or that it is made up of an admixture of new and old elements, of which the older, as far as the mammalian remains are concerned, are in as good condition as the newer. At any rate it contains rolled Acheulean implements, but was followed immediately by early Mousterian industries; it is not necessarily post-Acheulean. The fauna is a strange mixture of warm and cold forms; the leading members of both types appear. Musk ox occurs, and reindeer appears for the first time. This sounds like a mixture of everything that might be found in the end of the Summertown-Radley upper-gravel phase and the Wolvercote Channel, including rare reindeer; the altitudes are much the same. Perhaps it is most accurately described as embracing, with the succeeding Crayford brickearths and Coombe Rock, the whole of the Wolvercote Channel series. The phase is thus partly continuing Acheulean history, partly transitional to the Mousterian, and is brought to a sudden close by the Coombe Rock—" Warp "—Upper Chalky Boulder Clay episode.

(4) *Crayford Brickearth and Coombe Rock.*—This presents a compact group of facts. It is separated from the Taplow Terrace phase by a marked period of degradation, which does not seem to have worked so far up the river system as Oxford. The lower part of the brickearth is evidently Mousterian. Some authors regard the upper part as Aurignacian. In the middle of the series is a seam of shells, notably *Corbicula fluminalis*. This, like the interpolation of the remarkably cold or arctic flora in the middle of the Hoxne Brickearths, is one of those strange and bewildering little changes which some authors would wish to magnify to glacial or semi-tropical extremes, while others dignify them with the term oscillations, and yet others pass them over as ecological curiosities. The last are probably most nearly right but, even if we do not allow such facts to rule a broad correlation, we must not forget them. At best all our sequences are incomplete, and we cannot expect every event to be recorded everywhere. With reference to the Upper Thames the brickearths certainly represent part of the Wolvercote Channel. The upper limit in both is the Coombe Rock phase, which in Oxford continued during a considerable part of the subsequent re-excavation of the valley, including one marked pause. The phase seems to have ended, at least in an acute form, before the Flood Plain gravels were formed. It is probable that

the Crayford Brickearths cover, and owe some of their material to, a considerable part of this excessive degradation of ready-formed gravels in the upper river. They probably afford the best example of upper river erosion leading to, and being contemporary with, marked aggradation in the lower reaches.

5. In the absence of any special evidence nothing need be said here of the Flood Plain and lower gravels. In this respect the information available in the Lower Thames, thanks to the work of Mr. Hazzledine Warren and others, stands by itself.

The following summarizes what has been said :—

	UPPER THAMES.		LOWER THAMES.	
(*k*)	Present Day.			
(*j*)	River alluvium.		River alluvium, buried channel.	Neolithic.
(*i*)	Channel filled. *Glacial*, not in Thames Valley.		Lea Valley, Ponders End, etc., Flood Plain Gravels	Upper Palaeolithic.
(*h*)	Continued downcutting, excavation of deep channel.			
(*g*)	Warp, hill washes, downcutting.		Downcutting, Coombe Rock	U. Mousterian to Mousterian.
(*f*)	Warp. U. Chalky Boulder Clay. *Glacial*, not in Thames Valley.		Coombe Rock	
(*e*)	Land surface.		U. Crayford Brickearth.	
	River Clays. Sands and peat.	Mousterian.	L. Crayford Brickearth.	(Early) Mousterian.
	Lower Gravels.	Wolvercote Channel. Late U. Acheulean Micoque.	Taplow Terrace.	(Late) U. Acheulean.
	Upper Summertown-Radley Terrace, warm. —Erosion— Lower Summertown Terrace, cold.	U.–L. Acheulean.		
			Swanscombe series, or Middle (Acheulean) and Upper part thereof.	Clactonian, L.–U. Acheulean.
(*d*)	Moreton Drift, Wolvercote Channel, derived Chellean.		Kimmeridgic Boulder Clay of Essex. *Glacial*.	
(*c*)	140–100 foot Terrace Group.		Gravels such as Caversham, Dartford Heath, Boyn Hill (in part).	Chellean.
	Redeposition of Plateau Drift in high terraces throughout Thames Basin.			
(*b*)	Plateau Drift. *Glacial*.		?	
(*a*)	No detailed record of Pliocene times preserved.			

Plateau Drift.—Before concluding these notes I should make some reference to an aspect of the correlation of Norwich Brickearth and Oxford Plateau Drift which cannot be ignored.

The Plateau Drift might be accounted for by normal processes of land ice were it not for certain facts which were set out in a recent paper (*Quart. Jour. Geol. Soc.*, 1929, 359 et seq.). The presence of any erratics from west, north, or east is understandable, although it is difficult enough to envisage how the transportation of material from Scandinavia to a height of some 600 feet near Oxford was effected. Suspicion was thrown on a pure land ice hypothesis when it was proved by eminent geologists that material from Devon and Cornwall is a common constituent of the Oxford Plateau Drift. I have found it impossible to avoid their conclusion, and it must be met. Since 1929 I have reinvestigated the "obvious" solution that this material was derived from the Bunter beds of the Midlands, but without reassuring success. It cannot be over-emphasized, especially with regard to the discussion printed with the paper just mentioned, that the rocks are subangular and occur up to the size of a man's fist, and larger; they are neither sand grains nor rounded beach pebbles.

One has only to read Mr. Groves' paper on the unroofing of the Dartmoor granite to see that no trend of such material has taken place before Pliocene times, whatever distribution the finer detritus may have had. Mr. Groves holds out little hope for the Pliocene proving more helpful, either in his paper or in our correspondence. Recent discoveries of a greater extent of the Red Crag sea than was formerly realized (referred to in *Quart. Jour. Geol. Soc.*, 1929, 359 *et seq.*, especially 383, 385), do not solve the problem and the difficulty remains. At the moment some mystery surrounds the origin of the Plateau Drift, a problem which Miss Tomlinson has likewise found, and the possibility of submergence at some previous and unknown period, or at or about Norwich Brickearth times, coupled with extensive glaciation, cannot be ruled out. Not until detailed examination has been completed over most of the south and west of England, and much of the Midlands, can we expect a full body of evidence. Perhaps it is not without significance, however, that Professor Boswell's definition of the Norwich Brickearth (op. cit., p. 109) is "a boulder clay possibly produced by an ice sheet melting out in water".

SUMMARY AND CONCLUSIONS.

Certain facts have emerged from recent work on Pleistocene problems which allow us to review the position and to mark certain dating points in the correlation of British glacial deposits.

The glacial phases seem to be correlated as follows :—

	E. ANGLIA.	YORKSHIRE.	U. THAMES AND EVENLODE–STOUR.	L. THAMES.
4.	Brown B.C. of Hunstanton.	Newer Drift ?	[Contemporary cold phases in flood plain deposit.]	
3.	Upper Chalky B.C.	Hessle B.C.	[Warp, Coombe Rock, hill washes.]	"Trail."
2.	Chalky-Jurassic B.C.	Purple B.C.	Moreton Drift–Wolvercote Terrace.	B.C. of Essex.
1.	Norwich Brickearth.	Basement Clay.	Plateau Drift.	?

The legends in brackets refer to deposits contemporary with and equivalent to actual boulder clay outside the Thames Basin.

Interglacial beds contemporary with Palaeolithic cultures may be grouped between the glacials as shown in foregoing tables.

The Thames Basin has its own difficulties, towards the solution of which suggestions are made, based on certain "fixed points" and on the principle that erosion in the upper part of a river may be contemporary with, and will probably cause, aggradation in the lower reaches.

The following phases of erosion and aggradation are thus suggested, based on the table given above :—

	Upper Thames.		Lower Thames.
		Phase VII : Balanced.	
(*h*)–(*k*)	Deep erosion followed by deposition, with some erosion and oscillations. U. Palaeolithic to Present Day.		
		Phase VI : Unbalanced.	
(*f*)–(*g*)	Marked erosion during Coombe Rock times.	Mousterian.	Further aggradation of Crayford Brickearths, then erosion in Coombe Rock times.
		Phase V : Unbalanced.	
(*e*)	Aggradation of Wolvercote Channel.	Late Acheulean–Early Mousterian.	Marked erosion then aggradation of Crayford Brickearth as aggradation ceased in Upper Thames.
		Phase IV : Unbalanced.	
	Aggradation to 50 foot terrace for filling of Wolvercote Channel.	Late Acheulean.	Erosion, then building of Taplow Terrace.
		Phase III : Unbalanced.	
	Erosion, followed by Summertown Terrace aggradation.	Acheulean.	Aggradation of Swanscombe series.
		2nd Boulder Clay.	
(*d*)	Moreton Drift : deposition of outwash as Wolvercote (50 ft.) Terrace.		Kimmeridgic Boulder Clay of Essex.
		Phase II : Balanced.	
	Erosion of 50 feet of rock.		Erosion, amount unknown.
		Phase I : Balanced.	
(*c*)	Redeposition of Plateau Drift in high terraces, culminating in the Lower Palaeolithic 140 feet and 100 feet stages throughout the river.		
		1st Boulder Clay.	
(*b*)	Plateau Drift.		?
(*a*)	No detailed record of Pliocene times preserved.		

In conclusion I wish to express my thanks to Professor W. J. Sollas and to Professor P. G. H. Boswell for their advice and criticisms, which have been adopted and incorporated before these notes went to press.

APPENDIX.

The following letters, addressed to Professor Sollas, give the latest views of the Rev. Professor Breuil on the classification of the Coombe rocks and have a special interest as bearing directly on the results obtained by Dr. Sandford :—

8th September, 1931.

I wrote briefly on the Coombe rock in relation to the implements of Crawford and Barnsfield pit (Swanscombe), first, some three years ago, in a Summary of my work and bibliography, and next in an inaugural lecture the year after. Last year I communicated a short paper on the *three* Coombe rocks to the meeting at Portsmouth, but I do not know whether this was published. Colonel Palmer could tell you. In this paper I stated that there was important solifluxion in the Thames Valley, 1°. First, in *Mindel* times, after the formation of the 137 foot terrace and before the formation of the 100 foot terrace, as shown by the fact that the old Chellean and Clacton I implements found at the bottom of the Barnsfield pit which had been previously broken and striated, occur mixed with Clacton II implements which are entire and absolutely fresh ! This Coombe rock is now disintegrated, at least in this pit.

2°. Next the *Riss Coombe rock*, classically represented at Crayford, Grays, and Baker's Hole (Northfleet). This cuts into the 100 foot terrace and the 50 foot terrace, covers the Acheulean and Levallois I, II, has broken these implements, and extends down to the lower deposits of the *low terrace* at Crayford. (Crayford is *not* contemporaneous with the 50 foot terrace of the Upper Thames ; in the Riss-Würm interval the subsidence of the land led to the formation of thick fluviatile deposits containing *Corbicula* at a time when the Hippopotamus bed was being formed in the low terrace of Summertown, Oxford.) The lower fluviatile brickearth of Crayford belongs to the end of the Riss, and covered the hunting-grounds of Levallois III.

3°. In the Würm I time there was formed in the Tertiary country around London the deposit known as the Trail, and this destroyed in many cases the Ponder's End deposits, now only preserved in lateral valleys. The upper, cold brickearth of Crayford contains, beneath the Trail, Levallois blades, probably VI or VII.

The Trail was formed during an elevation of the land, and extends down into the sunk channel. Contemporaneous with the Trail is another Coombe rock which extends over the Chalk lands.

I regard the Coombe rock as the chalky facies of solifluxion in the Chalk country. It is a "periglacial" phenomenon of great importance in your country, which lies so near the glacial front ; in the north of France it is also met with, but naturally not so strongly marked. It furnishes the explanation of the great semi-fluvial deposit of the Montières low terrace (upper low terrace not the lower) on which the warm horizon of Levallois IV and Micoque (not mixed) rests at

Etouvy. To this time belong the chalky rubble of Sangatte on which the younger loess (Levallois V–VII) rests, and which overlies the well-known beach—unjustly called " cold ", for the shells are the same as to-day, with two warmer and two colder species.

I attribute the Sangatte beach to the same Riss-Würm subsidence as the brickearth of Crayford.

H. Breuil.

24th September, 1931.

I did in fact publish in 1926 a short paper in *Man*, but since then my views have evolved further and I have now, I believe, a more definite and certain position, so you will consider the note in *Man* as a first step, taken when I regarded the Chalky Boulder Clay and the Coombe rock of Northfleet as Würm. I am now convinced that they are *Riss*. The Coombe rock of Northfleet (Swanscombe) has *removed* the old Levallois I, II workshop (not true Moustier); it is older than or contemporaneous with the bottom gravel and lower brickearth of Crayford, which contains Levallois III. The brickearth (fluviatile) of Crayford with a cold fauna at the base and at the top (which is marked by some late Levallois (VI–VII) blades), gives evidence of warm conditions in its middle layers.

The fluviatile brickearth is succeeded by the Trail (gravel, very contorted following the slope) which I regard as Würm.

In the terrestrial brickearth, which is a wash of loess or loam, no bones or shells are to be found ! . This formation has no date, and I have seen such deposits on slopes of recent date in northern France, sometimes of considerable thickness.

Some twenty or thirty years ago, when digging in the Grand Pressigny, H. Martin found in such a formation a succession truly wonderful, a curious inverted stratigraphy, with Neolithic at the bottom, Moustier–Levallois in the middle, and Acheulean at the top. This inversion was the result of successive washings of the adjacent plateau. The Neolithic alone was in situ, or washed from the superficial loam of the plateau, then later the middle and lower deposits were washed away and redeposited on the slope.

H. Breuil.

Batholiths of Southern Rhodesia.

By A. M. Macgregor.

(Published by permission of the Director, Geological Survey of Southern Rhodesia.)

THE problem of the formation of the larger masses of granite is receiving considerable attention by geologists at the present time, and it is hoped that the following observations and surmises regarding the granites of Southern Rhodesia may be of more than local interest. Southern Rhodesia forms part of a typical Pre-Cambrian shield and consists, apart from younger formations, of about 74,000 square miles of granite and gneiss intrusive into 13,000 square miles of older rocks. The area has undergone tremendous denudation, which took place mainly during the Pre-Cambrian, and the structures exposed, therefore, are different from those seen in regions where only the peaks of granite masses have been uncovered.

At the outset I wish to thank Mr. H. B. Maufe for permission to draw from the store of information, much of it unpublished, which he has collected in the Geological Survey Office at Salisbury, and also for having the accompanying illustrations drawn in the office. This information includes both petrological and structural data, but it is only the structural aspect of the problem which it is intended to discuss here.

Relation between the Granites and the Older Rocks.

The surface relations are shown in the small-scale geological map of the central and eastern part of the Colony (Fig. 1). The term "Basement Schists" is used in a loosely stratigraphical sense to comprise all rocks older than the adjacent granite masses. The known trend lines in the belts of Basement Schist are shown on the map by continuous lines. Broken lines indicate that the trend lines are doubtful or inferred.

The belts of Basement Schist are believed to become narrower downward and to be underlain everywhere by granite or gneiss, for reasons which may be summarized as follows:—

(i) The larger schist belts occupy the central portion of Southern Rhodesia between Bulawayo and Gatooma. Approaching the borders in all directions the belts become smaller and more widely separated. In the central area the regional metamorphism is of a low order, and is characterized by the intense development of carbonates in rocks associated with the larger granites. Within the belts some of the rocks may be younger than these granites. Among these augite is either quite fresh or chloritized, and is amphibolized only in the contact zones of the granite. The felspars are saussuritized or sericitized. Although the dip of the strata generally exceeds 45 degrees the rocks frequently show no evidence of compression, calcite vesicles in the lavas are often undeformed, and the sediments show little or no cleavage (1).[1]

In the smaller belts situated farther from the centre of Southern Rhodesia, the metamorphism is of a higher order (2). Primary augite is absent, giving place to hornblende. Biotite, andalusite, and cordierite are common. Still farther afield staurolite, kyanite, and garnet become frequent in the north, and garnet- and sillimanite-gneisses are found in the south-east, where the schist belts are diffused in gneiss (3). The map shows that the proportion of granite to schist country is lowest in the central part

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[1] Figures in parentheses refer to list of Notes and References at the end.

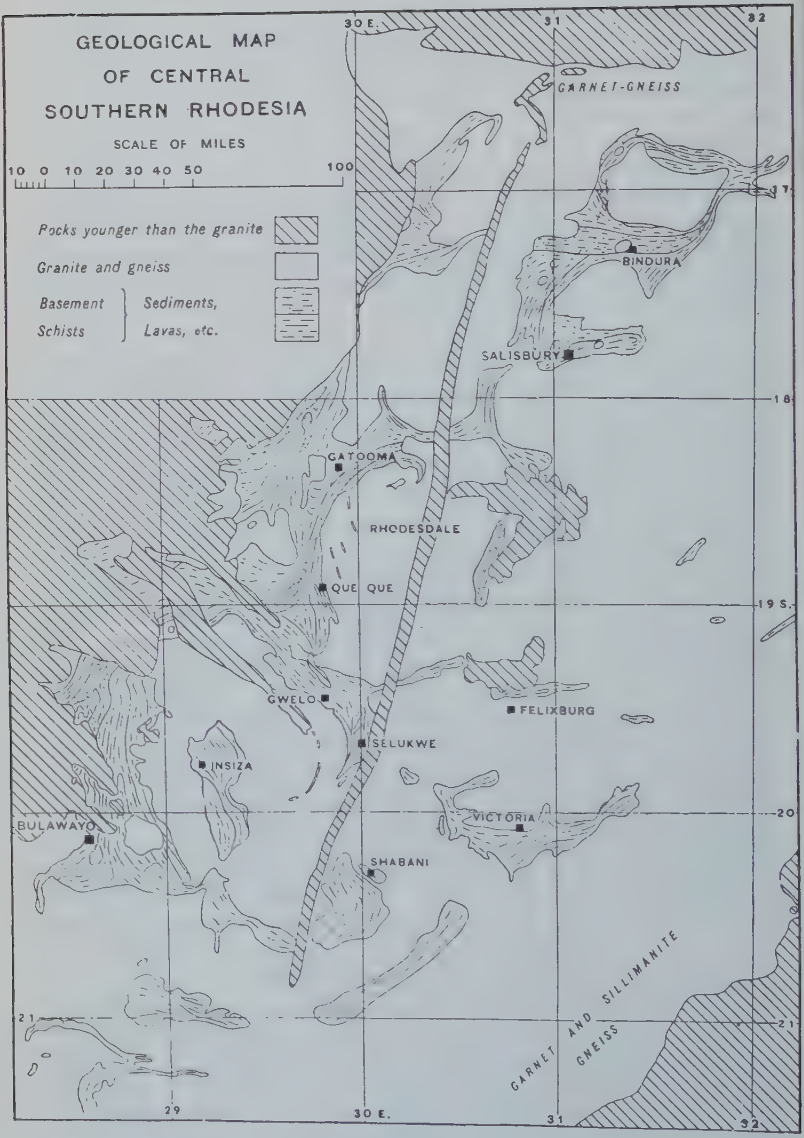

Fig. 1.—Geological Map of Central S. Rhodesia.

of the area where the grade of metamorphism is lowest, and increases outwards with the metamorphic grade. In so far as the grade is indicative of the depth of erosion the above facts suggest a downward diminution of the belts of schist.

(ii) The larger schist belts have a synclinal or synclinorial structure, being formed mainly of lava-flows in which the upper and lower surfaces are clearly discernible (4). There is frequently a sedimentary belt overlying the lavas near the centre. The general tendency for the trend lines to run parallel with the contacts of the larger granite masses is evident on the map. In vertical cross-section it is probable either that there is a similar parallelism between the dips of the strata and the granite contact, or else that the granite has invaded the schists in the deeper parts of the synclines. The presence of transgressive stocks of granite in some of the synclinal belts suggests that the latter has occurred.

The "Basement Schists", therefore, appear to be the remains of the crumpled roof of a continental granitic shell which, however, is certainly not a single granitic intrusion but a composite mass formed of granites of various characters and of different ages.

Before proceeding to discuss the granites a little more may be said regarding the folding of the older rocks. In the larger belts alteration has taken place mainly by pseudomorphous replacement. Intense shearing is limited to narrow, widely spaced zones, and does not affect the body of the rocks as a whole. Not only, however, are the dips uniformly at a high angle, but the axes of the drag folds also pitch very steeply (5). The axes of the main folds appear to do the same. In some cases the structure appears to resemble the folding produced in a piece of cloth floating upon water when a small weight is placed upon it.

The rounded outlines of the larger granites, conformable with the trend of the older rocks, do not suggest a directional pressure from any particular point of the compass. There is little, therefore, to connect the structures with the roots of a mountain chain, or of intersecting mountain chains, formed by lateral compression. On the other hand, the pattern may be compared with the ring-fault structure of Iceland, or with the Vredefort door-knob dome (6).

STRUCTURE OF THE GRANITE MASSES.

None of the larger granites in Southern Rhodesia, so far as the writer's knowledge goes, shows either evidence or suggestion of the possession of a floor. Granite laccoliths or lopoliths appear to be totally absent. Judged, therefore, by the criterion of apparent bottomlessness the masses are to be classed as batholiths.

The first use of this term is conveniently summarized by a report recently circulated from Washington (7) as follows: "Suess originally defined batholiths as stocks or shield-shaped masses that originated by melting and assimilation by the intruding magma of a portion of the invaded formation." This is a clear definition, and it is only necessary to determine by field observation to what bodies it can justly be applied.

The word, however, has been used in a much wider sense (8) to include all large granite masses which have no visible floor. In this sense Professor R. A. Daly (9) adopted the term and re-defined it to include all masses exceeding the arbitrary minimum surface area of 100 square kilometres, and showing evidence neither of a floor nor of downward narrowing. He includes as a first characteristic their location in orogenic zones. As typical batholiths he cites a number of uniform granite masses which appear to have been formed by bodily replacement of part of the intruded formation, a process which he believes to have been accomplished by stoping. At present the word batholith is applied indiscriminately to every large intrusion which is not regarded as a lopolith or a laccolith, a sense which is so vague that a term to convey it is hardly justified.

In Southern Rhodesia there are several granite masses which seem to have been formed by bodily replacement of the older formations by a differentiated magma, and are therefore batholiths in Daly's sense ; but in addition there are large masses of migmatitic granitic gneiss, formed apparently by a different process in which assimilation, palingenesis, played an important part. These masses are closely comparable with the migmatites of Southern Finland (10) and may be accepted provisionally as batholiths in Suess's sense. Whether or not the explanations of the two methods are regarded as correct, the field evidence seems to indicate that two distinct types of intrusion are present, which may be designated *Daly-batholiths* and *Suess-batholiths* respectively.

Some of the characteristics which appear to distinguish these two types are tabulated below :—

Suess-batholiths.	*Daly-batholiths.*
(*a*) Intrusive rock variable in character, often even in the hand specimen.	(*a*) Intrusive rock of uniform character, or of various types with clearly definable petrographic characters, and definite mutual contacts.
(*b*) Included fragments common. They may be of any size and in any stage of assimilation. They are usually traversed by acid veins which may show ptygmatic folding.	(*b*) Included fragments rare.
(*c*) Large roof pendants are characteristic.	(*c*) Roof pendants rare.
(*d*) Evidence of assimilation frequent throughout, but particularly near margins.	(*d*) Evidences of assimilation of fragments rare.
(*e*) Margins of the mass sometimes very intricate on a small scale, but trend of surrounding rocks usually approximately parallel to the contact.	(*e*) Margins of mass usually cleanly and sharply defined, often transgressive.

Suess-batholiths.	*Daly-batholiths.*
(*f*) Grain usually fine, though large ragged felspars may be present.	(*f*) May be coarse-grained or porphyritic.
(*g*) Micrographic structures rare.	(*g*) Micrographic structures common where grain sufficiently fine to show them.
(*h*) Form rolling country with few exposures.	(*h*) Form broken country with numerous granite domes and tors.

A few masses show characters of both types. In these cases it is possible that closer examination will show that a Daly-batholith has almost replaced a Suess-batholith.

THE SUESS-BATHOLITHS.

The foregoing list of characters ascribed to these intrusions has been based essentially on the two masses situated north-eastwards and south-westwards of the town of Gwelo. These masses have areas of approximately 3,000 and 4,000 square miles respectively, and each is bounded by older rocks along about nine-tenths of its periphery. These masses are both formed mainly of gneissic granite and migmatite, but the continuity of the gneissic structure, which varies in different parts from a feeble parallelism of the biotite flakes to that of a migmatite, is broken not only by large and small inclusions, but also by bodies of differentiated, massive, or porphyritic granite, which are regarded as later Daly-batholiths. A description of these relations is given by the late P. A. Wagner (11). Similar granite masses occur throughout the Colony, but their mutual limits are practically unknown.

These two masses lie in a region of comparatively low relief, and little definite information about the nature of the contacts is to be gained except in the river beds, and in mine workings at one spot. A plan of the contact in the neighbourhood of Que Que is shown in Fig. 2. The contact here is more irregular than is usual, but is none the less instructive in respect of the numerous inclusions of older rock in the granite on the one side, and the numerous small intrusions of granite on the other. The largest of these minor intrusions has a length of about 6 miles and a maximum width of half a mile. It is separated from the main granite by a belt of older rocks about half a mile wide. Felspathic quartzite outcrops at both ends of this mass and there can be little doubt that the granite has been formed by selective, syntectic replacement of the arkose. The underground form of the contact of the main mass is exposed in the workings of the Phœnix Mine to a depth of 3,400 feet (shown in Fig. 3). The granite adjacent to the talc-carbonate schist is a fine-grained, very leucocratic type which at a distance of 150 feet or more from the contact passes gradually into migmatite. *Lit-par-lit* injection has not been observed. Unfortunately, the underground form of the contact of the narrow western granite is not known.

The contact in this neighbourhood has been described rather fully because the evidence is unusually complete, but in some important respects the structure is not typical. The fine-grained acid margin is quite a local feature, and is probably not a chilled edge. Chilled edges seem to be totally absent from the contacts between the gneiss and the roof, walls, and inclusions. The inward dip of the contact at a moderate angle is also unusual. Judging by the foliation of the rocks on either side of the contact at various points round the masses outward dips are more frequent than inward dips, though the latter are not uncommon and appear to occur on the east side of the masses as well as the west. The form

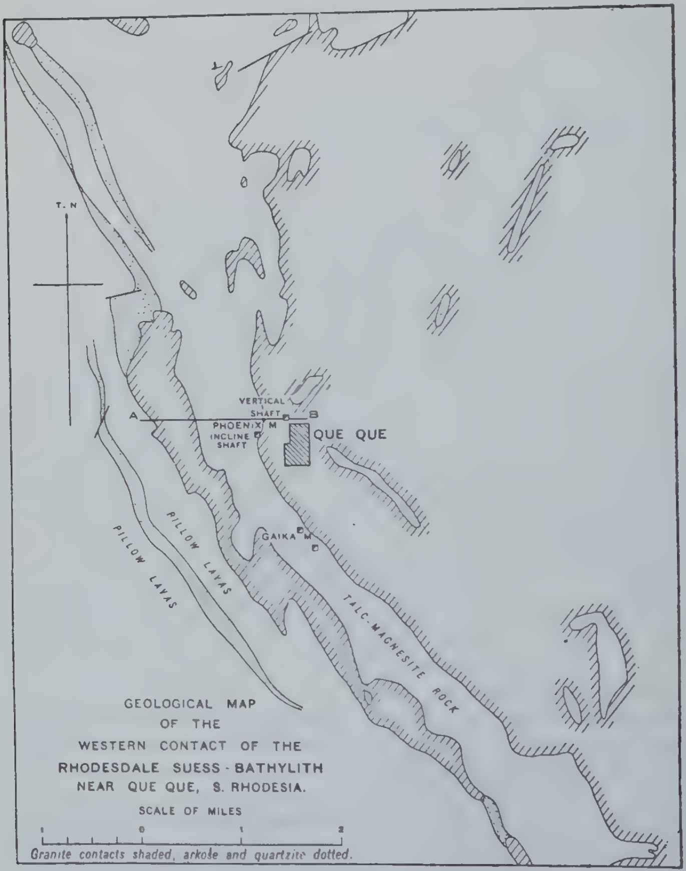
T. N
A
B
VERTICAL SHAFT
PHOENIX M
INCLINE SHAFT
QUE QUE
GAIKA M
PILLOW LAVAS
PILLOW LAVAS
TALC-MAGNESITE ROCK
GEOLOGICAL MAP OF THE WESTERN CONTACT OF THE RHODESDALE SUESS-BATHYLITH NEAR QUE QUE, S. RHODESIA.
SCALE OF MILES
1
0
1
2
Granite contacts shaded, arkose and quartzite dotted.

Fig. 2.—The Western Contact of the Rhodesdale Bathylith.

of the contact appears to be dependant upon the folding of the older rocks, and their amenability to granitization.

The same consideration applies with regard to the larger inclusions in the batholiths. In the Rhodesdale batholith northwards of Gwelo, the larger inclusions are composed mainly of quartz derived probably from quartzite, or of serpentine or other ultrabasic rock. In the north-western portion of this area the quartz forms lenses traceable discontinuously in parallel lines trending in a south-south-westerly direction. The serpentine inclusions are distributed less regularly.

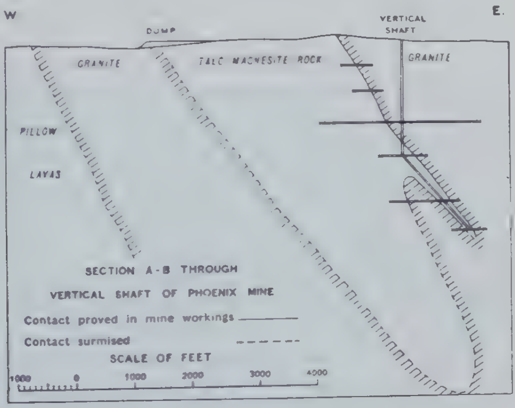

Fig. 3.—Section through the workings of the Phœnix Mine, S. Rhodesia.

The batholith to the south-west of Gwelo has an annular form surrounding the Insiza gold belt, an area about 50 miles by 20, which is perhaps a sagged portion of the roof. The gold belt has not yet been studied in detail. In the granite east of this is the chain of inclusions forming the Ghoko hills. This group has the form of an inverted C and trends approximately parallel with the eastern contact of the mass for a distance of about 40 miles. Wherever the dip of the schists, mainly banded ironstone, quartz, and talc-schist, has been observed, it is steeply towards the centre of the curve.

Inclusions of rock of intermediate composition are usually broken up into small masses forming characteristically the darker components of the migmatites, and vary, according to the amount of alteration which they have undergone, from massive amphibolite to pale grey wisps only a little darker than the granite matrix. Different stages in the change can sometimes be followed under the microscope. Quartz and felspar have filtered in among the hornblende crystals, gradually separating them. The hornblende seems to dissolve and biotite, formed by reaction, to be precipitated.

The outline of the inclusions remains sharp up to a point, but they appear to have partly melted and at a certain stage to have had a lower viscosity than the enclosing magma. This is shown by the behaviour on distortion of aplite dykes injected at an early stage into the inclusions. So long as the viscosity of the inclusion is greater than that of the acid dyke, adjustment to directional pressure takes place in the latter, which becomes deformed into lenses. In other cases the dyke is folded ptygmatically, indicating that at the time the movement occurred, the viscosity of the dyke exceeded that of the surrounding inclusion.

Rocks with a composition approaching that of granite are rarely found as inclusions.

DALY-BATHOLITHS.

In so far as it is agreed that these batholiths in their complete form close upwards to a point or to points, the distinction according to area between stocks and batholiths, a distinction which is dependent only upon the depths of erosion, is arbitrary and misleading. It will simplify the discussion, therefore, if the term Daly-batholiths is made to include all stocks which appear to widen downward, however little of them is exposed.

Stock-like masses formed of differentiated granite are very numerous in Southern Rhodesia, and penetrate both the migmatic gneissic granites and the schist. The clearest cases of these intrusions into the gneissic granite have been described by Wagner (11) in the region between 30 and 50 miles due south of Gwelo. In this area of some 800 square miles he describes five bodies of massive granite and two of porphyritic granite and regards them as intrusive, with one doubtful exception, which he says may be a local differentiate of the gneiss.

In several places small stocks are located on synclinal axes in the schists, where the tension involved in the folding appears to have afforded access for the intrusion. Examples of these are found in the small intrusions south and east of Bulawayo, at Bindura, and at Felixburg (12). In other cases there is evidence of domical uplift of the surrounding strata having accompanied the intrusion. This appears to have taken place with regard to the mass north-west of Que Que (13).

GENETIC RELATIONS.

It is outside the province of this paper to discuss whether or not the accumulation of heat generated by radioactivity in a large mass of granitic composition must in time give rise to a magma basin or Suess-batholith (14), but it is safe to conclude that such basins can only be formed under a very considerable depth of cover, and are therefore not likely to be exposed by erosion except among very ancient rocks in regions of prolonged denudation. It is a significant fact that the migmatitic gneisses are characteristically, if not exclusively, found in the Pre-Cambrian shields.

Assuming that migmatitic gneisses can be formed in a mass of mixed rock of general granitic or dioritic composition by movement of the isogeotherms, then the following results may be postulated.

The first result will be a division of the rock-mass into the following zones which gradually move outwards from the source of heat:—

(*a*) Zone of no fusion.
(*b*) Zone of interstitial fusion—fritting.
(*c*) Zone of incomplete fusion—migmatitic gneiss.

Differentiation taking place in the lower part of zone (*c*) by settling to greater depths of undissolved heavier minerals will lead to the development of a zone (*d*) of completely fused granitic magma. The magma of zone (*d*) will therefore have a lower specific gravity and probably a higher temperature than the incompletely fused magma of zone (*c*). This difference of specific gravity in the fluid though viscous medium will have the effect of accentuating by slow movement any initial unevenness in the dividing surface. In domes or cupolas in this surface Daly-batholiths may originate by the development of fountain-form convection currents, and rise slowly at first where differences of specific gravity and temperature are slight, but more rapidly at higher levels where these differences are greater, and the force of the currents is increased by the fall of heavy stoped fragments from the walls, and the development of two-phase convection in the centre. By these currents newly arrived hot magma is brought into contact with cold virgin rock, freshly exposed by the removal of the last stoped fragment. So the stoping continues until the viscosity, increased by the loss of volatile constituents in the zone of fracture, prevents further scaling, but not the settling of the separated fragments. The stoped fragments will fall so long as their specific gravity differs appreciably from that of the enclosing magma. As they become assimilated the difference of specific gravity will decrease and their diffused remains may be swept inward towards the central up-cast current, or unite with the remaining migmatitic gneiss which they influence in cooling.

Professor Arthur Holmes explains the formation of batholithic masses by the process of crystal stoping (14) depending upon fusion at the interstices and not on fracture due to a local great difference of temperature. In the course of his argument he concludes that the zone of partial or interstitial fusion (fritting) will be thin. The absence of a thick zone of fritting, except in the arkoses and pyroclastics of approximately granitic composition, is a characteristic of the contacts. In many cases it has probably been obscured by sliding of parts of the roof over and through this zone, bringing comparatively unmetamorphosed rocks into contact with the migmatitic gneiss.

There seems to be no mechanism whereby a fluid magma at great depth can transmit a thrust to solid rock, but at moderate depths the apex of a tall Daly-batholith invading rocks of greater specific gravity may clearly exert great outward and upward hydrostatic pressure upon the enclosing country rock, giving rise to a domical uplift of the surrounding strata, or thrusting upwards a plug of solid rock bounded by marginal faults up which ring-dykes may be injected.

NOTES AND REFERENCES.

(Southern Rhodesia Geological Survey is abbreviated to S.R.G.S.)

(1) B. Lightfoot, "The Geology of the Country around Gatooma," *S.R.G.S. Bulletin,* No. 5, p. 12. A. M. Macgregor, "The Geology of the Country between Gatooma and Battlefields," *S.R.G.S. Bulletin,* No. 17, p. 14. In the area north of Bulawayo, also, the rocks and amygdales are likewise mainly quite undeformed.

(2) These conditions of metamorphism are exemplified in the Victoria, Selukwe, Salisbury, Bindura, and Darwin schist belts. See Maufe, Lightfoot and Zealley, "The Geology of the Selukwe Mineral Belt," *S.R.G.S. Bulletin,* No. 3; H. B. Maufe, "The Geology of the Enterprise Mineral Belt, *S.R.G.S. Bulletin,* No. 7; Lightfoot and Tyndale-Biscoe, "The Geology of the Country West of Mount Darwin," *S.R.G.S. Bulletin,* No. 10.

(3) Annual Report of the Director, *S.R.G.S.*, 1926, pp. 5 and 6.

(4) The structure of the pillow lavas has been found particularly useful. See *S.R.G.S. Bulletin,* No. 11, pp. 18 and 19 (in line 17 omit "and lower"), and *S.R.G.S. Bulletin,* No. 17, chap. ii.

(5) Maufe and Macgregor, "Second Interim Report on the Geology of part of the Bembesi Basin," *S.R.G.S. Short Report,* No. 9, pp. 8 and 9; A. M. Macgregor, "The Geology of the Country between Gatooma and Battlefields," *S.R.G.S. Bulletin,* No. 17, p. 31, and pl. v, figs. 1 and 2. Steep folding is to be seen among other places at the Sebakwe Poort near Que Que and at the Salisbury Kopje.

(6) E. B. Bailey, "Iceland—a Stepping Stone," Geol. Mag., LVI, 1919, 466–77, or Émile Haug. *Traité de Géologie,* p. 320; L. T. Nel, "The Geology of the Country around Vredefort," *Union of S.A. Geological Survey,* 1927.

(7) Exhibit "A". Report of the Committee on Batholithic Problems appointed by the Board of Geology and Geography of the National Research Council of the U.S.A., Washington, 1930.

(8) Adams, F. D., "Structure and Relation of the Laurentian System in Eastern Canada," *Quarterly Journal of the Geological Society,* lxiv, 1908, 127–48.

(9) R. A. Daly, *Igneous Rocks and their Origin,* pp. 89–116, New York and London, 1914.

(10) J. J. Sederholm, "On Migmatites and Associated Pre-Cambrian Rocks of South-West Finland," *Bulletins de la Commission Géologique de Finlande,* No. 58 and No. 77, Helsingfors, 1923 and 1926.

(11) P. A. Wagner, "The Geology of a Portion of the Belingwe District," *Trans. Geol. Soc. of S.A.,* xvii, 1914, 39–54. For further information regarding the southern batholith see B. Lightfoot, "The Geology of the Selukwe Mineral Belt," *S.R.G.S. Bulletin,* No. 3, p. 51; A. M. Macgregor, "The Geology of the Country around the Lonely Mine," *S.R.G.S. Bulletin,* No. 11, p. 50, and "The Geology of the Diamond-bearing Gravels of the

Somabula Forest," *S.R.G.S. Bulletin*, No. 8, pp. 13–14; and F. E. Keep, "The Geology of the Shabani Mineral Belt," *S.R.G.S. Bulletin*, No. 12, p. 60. For the northern or Rhodesdale batholith, see A. E. V. Zealley, "The Geology of the Country around Gatooma," *S.R.G.S. Bulletin*, No. 5, pp. 17–21, and A. M. Macgregor, "The Geology of the Country between Gatooma and Battlefields," *S.R.G.S. Bulletin*, No. 17, p. 54.

(12) A. M. Macgregor, "The Geology of a small area east of Bulawayo," *Trans. Geol. Soc. of S.A.*, xxiv, 150–67. The Bindura granite is the country rock of some promising gold mines. Mr. Lightfoot states in a personal communication that the greater part of this mass is a normal grey granite, but that there is a marginal belt along the southern edge of gneissic granite crowded with inclusions of Shamva grit. A. M. Macgregor, "The Geology of the Felixburg Goldfield," *S.R.G.S. Short Report*, No. 18.

(13) A. M. Macgregor, loc. cit., *S.R.G.S. Bulletin*, No. 17, p. 60.

(14) A. Holmes, "Contribution to the Theory of Magmatic Cycles," GEOL. MAG., LXIII, 1926, 306–29.

The Stratigraphical Relations of the Red Rock at Hunstanton.

By F. L. KITCHIN, Sc.D., Ph.D., F.R.S., and J. PRINGLE, D.Sc.

I. INTRODUCTION.

IN his interesting paper entitled "The Petrography of the Hunstanton Red Rock"[1] Dr. R. H. Rastall arrived at conclusions regarding the relation of the Red Rock to the strata below and above it which are strikingly at variance with the result of our own examination of the series exposed in the well-known cliff-section at Hunstanton.[2] While we considered that the Red Rock has no connection with the underlying Carstone, though resting on it with apparent conformity, and that the junction between these contrasted beds marks a stratigraphical gap of great magnitude, we found no reason to recognize a break between the top of the Red Rock and the overlying Chalk. Dr. Rastall, on the other hand, impressed by the fact that there is a certain gradation and an appearance of conformability between the Carstone and the Red Rock, took this as showing continuity between these deposits, while the appearance of a physical break at the top of the Red Rock seemed to him to contradict our view that the top bed of the Red Rock and the Chalk above ("Sponge Bed") are inseparable geologically.

Dr. Rastall saw in our conclusions an example of the contradictions and confusions to which (as he asserts) the use of palaeontological evidence can lead. He decided to test the matter by means of a petrographical examination of the strata in question. "The primary object of the present investigation was to determine whether, and to what extent, petrographical methods could throw light on a problem in which palaeontology had led to confusing and conflicting results. Therefore, the first principle obviously was to ignore fossils altogether, and to rely solely on the mineralogical constitution of the strata."[1] Fossils might rightly be ignored while the investigation of the mineralogical constitution of the beds was in progress; but to exclude them finally and not to use their aid at all was, we think, to throw much too heavy a burden on the evidence provided by the minerals, so insufficient as the sole basis for the stratigraphical deductions made. In the present case the point at issue is the age-relation of contiguous beds in the succession, and stratigraphical dating is undoubtedly the essence of the matter. It is certain that, in the event of a conflict in the geological results obtained by applying the two independent classes of evidence, stratigraphical dating must ultimately be done by means of the fossils. Mineralogical assemblages cannot, in the nature of the case, provide the necessary chronological clues. In fact, Dr. Rastall has ignored the only means for ascertaining the positions of the separate beds with reference to the units of the standard Cretaceous succession.

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[1] GEOL. MAG., LXVII, 1930, 436–58.

[2] F. L. Kitchin and J. Pringle, "On the Overlap of the Upper Gault in England and on the 'Red Chalk' of the Eastern Counties": GEOL. MAG., LIX, 1922, 194–8.

In his introductory paragraphs, Dr. Rastall discovers "a curious misunderstanding which has crept into the literature". This concerns Judd's pronouncement that the Hunstanton limestone (Red Rock) is conformable with the Chalk above, and unconformable with the "Neocomian". The quotation of this statement by Professor Barrois in his account of the Hunstanton section,[2] and by ourselves in 1922, is thought by Dr. Rastall to be inappropriate, because the statement applied to the relations seen by Judd in Lincolnshire, not in Norfolk. Professor Barrois was fully aware of this; yet he cited this pronouncement quite aptly in support of his own conclusion, because on the local evidence he tabulated the Red Rock at Hunstanton as belonging to his zone of *Ammonites inflatus* (Upper Albian) and the underlying Carstone as Aptian, implying an unconformity there. The term "non-sequence" had not then been introduced for a case in which the relations at a junction, though apparently conformable, are in reality discordant.

We fail to see that there has been any misunderstanding. Judd included the Carstone of Norfolk and the "Upper Sands" of the Lower Cretaceous Series in Lincolnshire under the term Neocomian, as used by him. He described the Red Rock as graduating down into the underlying sands at localities in Lincolnshire, "exactly as at Hunstanton".[3] Yet he emphasized the magnitude of the unconformity at the base of the Red Rock without excepting Hunstanton from the generalization. It is only necessary to read his papers and to look at his geological sketch-map[1] to feel certain that if he had thought the relation of the Red Rock to the substrata to be different at Hunstanton, he would have said so, and that he would have been bound to comment on it. It is clear that Norfolk was not excluded by him as also showing a manifestation of the "grand unconformity" between the Upper and Lower Cretaceous Series in our eastern counties.[2]

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[1] Op. cit., 440.

[2] C. Barrois, *Recherches sur le Terrain Crétacé Supérieur de l'Angleterre et de l'Irlande*, Lille, 1876, 157.

[3] J. W. Judd, "On the Strata which form the base of the Lincolnshire Wolds," *Quart. Jour. Geol. Soc.*, xxiii, 1867, 231, 232.

In adopting the view that at Hunstanton there is in reality no continuity between the Carstone and the Red Rock, we did not claim that there was any novelty in this conclusion; and we thought it most fitting to cite in support those authoritative opinions expressed by Judd and by Barrois more than fifty years ago.

The whole of the comments and arguments in this introductory section of his paper come to a focus in Dr. Rastall's contention that at Hunstanton and Snettisham there is shown a continuous sedimentation without break between the Carstone and the overlying Red Rock, with the implication that these deposits are immediately successive in geological time. Unfortunately Dr. Rastall only states a portion of the problem; and by refraining from discussing the age-relations of these two deposits on the basis of the most appropriate evidence that is available, the stratigraphical and palaeontological evidence, he leaves the reader imperfectly informed, and his remarks consequently seem to us to be a misleading introduction to the parts of the paper which follow.

The use of the words "most appropriate" may perhaps be found provocative; but in the present case there is stratigraphical evidence available that is essential, beyond that which can be obtained by examining the Red Rock and its apparent relation to the Carstone only at Hunstanton and Snettisham. We cannot claim that there is any innate perfection and finality in palaeontological evidence. Its reliability as proof of geological age can only be ascertained by empirical observations and results, and finally established by long experience of uniformity in the matter of the vertical range and relative disposition in the strata of species or broader groups, as the case may be. Palaeontological facts which have stood this test of constancy become the most appropriate evidence for stratigraphical correlations, unequalled by any other kind of evidence whatsoever; and it is unnecessary to enter into any argument in support of this statement. Those palaeontological facts with which we are concerned in showing the age-relation between the Carstone and the Red Rock at Hunstanton have had their value for this purpose amply tested by long experience. When utilized in combination with stratigraphical observations, they form a much broader and firmer basis for conclusions than that upon which Dr. Rastall has taken his stand. They lead us to results quite different from those which he has reached by the consideration of the limited class of facts which he has been content to utilize as evidence. He has, in fact, imposed upon himself an unnecessary handicap by his restricted use of available evidence.

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[1] J. W. Judd, "Additional observations on the Neocomian Strata of Yorkshire and Lincolnshire, with notes on their Relations to the Beds of the same age throughout Northern Europe": *Quart. Jour. Geol. Soc.*, xxvi, 1870, pl. xxiii, fig. 3.

[2] Ibid., 331.

II. The Relation of the Red Rock at Hunstanton to the Carstone below it.

The assignment of the Carstone to the Aptian by the earlier investigators, on the strength of the ammonites found in its basal part, has been confirmed in recent years by Dr. L. F. Spath, who places this fauna in the *deshayesi* zone of the Lower Aptian. This fossiliferous basal bed may give condensed representation of more than one subzone or may be of *consobrinoides* age, containing derived fossils from underlying subzones. Some representation of the succeeding basal subzone of the Upper Aptian is possible, but it is not yet quite satisfactorily proved.[1] There is here no fossil-evidence for higher zones in the Carstone up to the basal part of the Red Rock, in which there occurs a fauna of ammonites and bivalves which shows clearly that there is nothing older than species of the *orbignyi* zone of the Upper Gault (Upper Albian) by which to date the lower part of the red beds with certainty. While it seems possible that the *cristatum* zone, the topmost part of the Middle Albian, may have some representation in the base of the red beds, the evidence for it is not conclusive. Between these two horizons characterized by fossils at Hunstanton there is a thickness of only about 35 feet of coarsely sandy and pebbly Carstone. At Snettisham the Carstone is probably thinner. Between the same horizons there are in other districts some eighteen or nineteen zonal subdivisions of Upper Aptian and Lower and Middle Albian age, showing a succession of distinctive ammonite-faunas and represented by hundreds of feet of strata, including the more slowly deposited clays of the Lower Gault.[2] A considerable part of the Lower Albian is without representation anywhere in this country, and its development is only known abroad. The Carstone in Norfolk cannot be considered to be more than a fragment of the Aptian succession, which is represented farther south by the Atherfield Beds, the Hythe Beds, the Sandgate Beds, and the Folkestone Beds.

How is it possible to reconcile these age-determinations, and the great hiatus they imply at Hunstanton, with the idea that the Carstone and the Red Rock illustrate continuous sedimentation without break? It might be suggested that the Carstone, in agreement with Dr. Rastall's view that the Red Rock is conformable and continuous with it, is really of Middle Albian age (Lower Gault), showing the development of that part of the Gault in a facies quite unknown elsewhere in England. It would then be necessary to suppose that the Aptian ammonites found in its basal part were derived into that position. But there is nothing whatever to support that view; and the determination of the Carstone as Lower Aptian accords perfectly with the fact that the Snettisham Clay, immediately underlying it, is of Barremian age, as shown by Dr. Spath's determination of its ammonitoid fossils.

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[1] See L. F. Spath, "On some Ammonoidea from the Lower Greensand," *Ann. Mag. Nat. Hist.*, **10**, v, 1930, 422, et seq.

[2] The succession of zones is summarized in tabular form in the *Handbook of the Geology of Great Britian*, London, 1929, 385 (Aptian zones), 393 (Albian zones).

Another suggestion might be that the Carstone represents the whole long period of time between the *deshayesi* zone and the *orbignyi* zone; but this view could not be seriously advanced, having regard to the nature of the Carstone as a deposit and bearing in mind the thickness and character of the variable strata (including the Lower Gault Clay) which elsewhere represent that time-period. We must also remember the total absence of much of the Lower Albian Series in all other parts of this country. It is necessary to recognize a gap somewhere at Hunstanton between the base of the Carstone and the lower part of the Red Rock. Stratigraphical considerations speak emphatically for the base of the Red Rock as the position of that gap.

These stratigraphical considerations concern the relation of that part of the Red Rock which belongs to the Gault, and its equivalents to the south and to the north of Hunstanton, to the substrata at the different localities. North-east of Cambridge, where concealed beneath the Fenland, the Upper Gault (Upper Albian), with possibly some representation of the *cristatum* zone at its base, has been shown in borings to rest upon a basal part of the Sandringham Sands, thus giving evidence of much erosion before its deposition.[1] At West Dereham these beds lie non-sequentially upon a thin remnant of the Lower Gault (Middle Albian) which may originally have been deposited in fuller development but now includes only a representation of the *dentatus* zone and the *mammillatum* zone at the base, though apparently not the intervening zones or any higher zone of the Middle Albian. At Mouse Hall ("Muzzle") the same relations are seen (Upper Gault on denuded Lower Gault), and there the Lower Gault is only 3 feet in thickness. The Upper Gault yields fossils of the *orbignyi* zone. Farther north, as at Grimston and Roydon, no basal Lower Gault has been found to be present, and the Upper Gault, now more calcareous, thinning, hardening, and showing the first signs of the introduction of red coloration, rests upon Lower Greensand which may or may not correspond exactly with the

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[1] J. Pringle, "On the Concealed Mesozoic Rocks in South-west Norfolk," "Summary of Progress" for 1922: *Mem. Geol. Surv.*, 1923, 128, 129, 132, 133.

Carstone at Hunstanton. At Dersingham this Upper Gault, showing more marked coloration, is reduced to 7 feet in thickness, and it becomes thinner still at Snettisham, where it rests upon the Carstone of Aptian age and has attained the character of the Red Rock.

In Lincolnshire the thin Upper Gault (as represented in the lower part of the Red Rock) rests at some localities upon Carstone (" Upper Sands ") comparable with that of Hunstanton, but in other places upon different members of the underlying Neocomian series, while farther north it passes on to Jurassic clays of Kimmeridge and Corallian age, and on to older beds. Judd, as we have already mentioned, described it as showing gradation at the junction into the Lower Cretaceous " Upper Sands " (Carstone) below it, just as at Hunstanton; but he gave convincing reasons to demonstrate that it passes transgressively over this sandstone and the underlying Neocomian series.[1] Jukes-Browne was of the same opinion. In spite of the appearance of passage between the Carstone and the Rock, he believed that the Red Rock creeps over the Carstone, which thins by erosion: " For there is a much greater thickness of Carstone in the Thoresway valley than there is at Acre House on the escarpment; and I regard its disappearance northward as a matter of overlap."[2]

Now, how can these facts be interpreted except by saying that the Red Rock (together with the Gault to which its lower part corresponds) is a transgressive deposit, passing unconformably over Lower Albian, Aptian, Neocomian, and Jurassic beds? If, at Hunstanton, it happens to lie upon the Carstone in such a manner that it appears to have parallelism of bedding and conformity with that sandstone, are we therefore to say that it rests conformably upon it? If the word " conformably " implies that these contrasted beds owed the lithological transition at their junction to original continuity of deposition, then it must not be used. We cannot here attempt suggestions for a revision of the existing stratigraphical terminology; but if it will satisfy Dr. Rastall, we can admit our inability to show that at Hunstanton the bedding of the Red Rock is not locally strictly parallel with that of the Carstone, although we may suspect that it is not. Whether the bedding is strictly parallel or not does not, however, seem to us a question of any significance, in view of the evidence for a stratigraphical break here discussed. We must insist that there is a non-sequence of great magnitude between these deposits, and that the missing strata are those of Upper Aptian and Lower and Middle Albian age (excepting possibly the very top of the Middle Albian), while recalling once more the fact that much of the Lower Albian has no representation elsewhere in this country. If, however, we substitute the late S. S. Buckman's term " non-sequence " for " unconformity " in describing the relations of these beds at Hunstanton, this does not support Dr. Rastall's contention that there was continuity of deposition between the Carstone and the Red Rock. It contradicts it emphatically.

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[1] Op. cit., 1867, 242.

[2] A. J. Jukes-Browne, " The Gault and Upper Greensand of England," *Mem. Geol. Surv.*, 1900, 305.

It will now be readily understood why we consider that Dr. Rastall's conclusions regarding the relation of the Red Rock to the underlying Carstone are vitiated by his neglect to discuss the age-relations of these deposits on the basis of much proper evidence that is available. In our opinion the facts upon which he has relied, the appearance of conformity between the Carstone and the Red Rock, and the results of a petrographical examination of the Red Rock, though this has been carried out so thoroughly and skilfully, have not sufficed to lead him to correct conclusions. The corrective lies in other, more vital evidence which he has not utilized in his argument. We do not understand why he should have omitted to discuss this evidence, so strongly opposed to his view, and to advance sufficient reason to rebut it.

If the relations of the Carstone and the Red Rock are not those of a continuous sedimentation, as thought by Dr. Rastall, but of an actual unconformity, as we believe to be clearly demonstrated, the occurrence in the Red Rock of mineral grains of the underlying Carstone is exactly what we should expect to find, whether derived as a result of exposure and denudation at some other locality or of a "working up" *in situ*. Indeed, it is a common experience to find constituents of the substratum, either rock-fragments, pebbles or nodules, or mineral grains large or small, in the lowest part of a transgressive series of beds, whether the conditions of deposition accompanying the encroachment were turbulent or tranquil. It is certainly rare to find the base of a transgressive series showing such an appearance of gradation at the junction as is displayed at the base of the Red Rock at Hunstanton, and it is difficult to form any definite idea of the exact conditions which could bring about this result.

Dr. Rastall gives reasons for thinking that the mineral grains in the Red Rock could not have been derived from the Carstone,[1] but we do not find these convincing. For instance, he states that "sand grains with a pellicle of iron oxide could not survive marine denudation and transport: they would be scrubbed clean by wave and current action, and there is no sign of this. The grains differ in no way at all from those of the undisturbed Carstone below". We can imagine that under certain conditions the grains might well have been derived without much scrubbing; they need not necessarily have travelled far or have suffered prolonged attrition. When we bear in mind the great gap in time between the formation of these two deposits, the repeated derivation of the mineral constituents from a common source of origin seems to us much more improbable than the view which we feel bound to adopt, namely, that in some manner the minerals were derived from the Carstone into the Red Rock. Dr. Rastall has pointed out how difficult it is to understand what the actual conditions could have been which produced such a peculiar deposit as the Red Rock, with its mingled coarse and very fine material. There certainly remains much to be explained regarding its mode of origin; but that it lies on the Carstone as the base of a transgressive series we feel no doubt whatever.

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[1] Loc. cit., 451.

III. THE RELATION OF THE RED ROCK AT HUNSTANTON TO THE LOWER CHALK ABOVE IT.

In considering the junction of the uppermost bed of the Red Rock with the basal part of the overlying Chalk, the so-called "Sponge Bed", and in estimating the significance of the physical break at this junction as bearing on the question of continuity in the succession, we feel bound again to attach great importance to evidence which Dr. Rastall has left out of account. It seems to us essential to ascertain, in the first place, the age-relation of these successive beds, and to utilize the evidence from which the surest indication of this relation can be obtained. This, again, is the evidence provided by the fossils. If is of importance to compare the fauna yielded by the uppermost red bed with that contained in the Sponge Bed, in order to find whether there are indications that the two beds belong to one narrow time-period or, perhaps, to successive zones in continuity, or even to separate zones having a stratal gap between them. In this manner only can we decide whether there is a non-sequence of any appreciable magnitude. It appears to us that there exists no other criterion so certain as a comparison of the fossils.

Dr. Rastall wrote: "As to the reference on palaeontological grounds of part of the Red Rock to the Lower Chalk, I have nothing to say."[1] We consider that he refrained from discussing a point of prime significance. A considerable period of geological time elapsed between the deposition of the lower part of the Red Rock and the laying-down of the Sponge Bed. It is therefore necessary to ascertain whether the faunal characters of the uppermost part of the Red Rock link this stratum more closely to the Upper Gault, as represented in the lower parts of the Red Rock, or to the Lower Chalk above. As we understand Dr. Rastall's account, his results lead him to the opinion that the top part of the red beds is the final development of the phase of sedimentation peculiar to the Red Rock; that the thin irregular seam of red clayey and ferruginous mud separating the Red Rock from the Sponge Bed is a solution-residue of the Red Rock, with added ferruginous material; that a certain amount of submarine denudation of the top of the Red Rock occurred before the deposition of the Sponge Bed began; and that there was a change in the constitution and sources of the mineral assemblage laid down during this pause, as shown by newly introduced minerals in the red seam. His careful examination shows him that the Sponge Bed marks a fresh phase of sedimentation, mainly calcareous, and that there is an absence there of the heavy minerals found in the Red Rock and in the thin red seam of clay.

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[1] Op. cit., 447.

Dr. Rastall's searching mineralogical examination has provided an informing supplement to our bare statement that there is a close lithological resemblance between the uppermost bed of the Red Rock and the basal bed of the uncoloured Chalk. This statement conveyed exactly the impression that we had gathered by an examination of the weathered surfaces in the cliff-section. We noticed that in both these beds the weathering produced a similar nodular aspect in the rocks; that concretionary segregations had resulted in the formation of the peculiar ramifying structures formerly called "*Spongia paradoxica*" in both beds alike, though not in the lower parts of the Red Rock; that the organic constituents, contributing so much to the lithology, included innumerable angular, broken fragments of *Inocerami* disposed in all directions, pointing to some special conditions of supply and accumulation common to both beds. While Dr. Rastall's mineralogical examination has enabled him to emphasize differences not evident from any superficial study of these strata, such similarities as we noted seemed to us to support with sufficient strength the idea of the stratigraphical unity of the two beds as revealed by the close similarity in their faunas. Though not much stress can be laid on the ramifying concretions which give such a curious character to these two beds, whatever may have been the period when these structures were formed, their presence suggests common qualities in the constitution and make-up of these strata, not shared by the lower parts of the Red Rock, facilitating the transport and segregation of the mineral matter which gave rise to the concretions. The result was certainly to increase the general lithological resemblance.

The fauna of the uppermost red bed is not that of the lower parts of the Red Rock, but is practically identical with that of the Sponge Bed. In other words it is a Lower Chalk assemblage. Among the bivalves found in both beds are *Inoceramus crippsi* Mantell, *Inoceramus tenuis* Mantell, and *Spondylus latus* (J. Sow.). There can also be seen on the weathered surfaces of both beds numerous portions of a Lithistid sponge, which appears to agree in its characters with *Nelumbia tuberosa* Hinde; and there are brachiopods common to these strata which are distinct from those of the lower red beds. All this is evidence pointing to continuity. S. Woodward's record of the rare occurrence of "*Ammonites varians*" in the Red Chalk may also possibly have significance in this connection, though it cannot now be checked. There is an absence of the Gault fossils found below, and the fauna of the Sponge Bed is in such a marked degree a continuation of that found in the uppermost bed of the Red Rock that we feel bound to regard these strata as intimately linked together. The special conditions of accumulation which resulted in the promiscuous deposition of angular *Inoceramus*-fragments in the top part of the Red Rock persisted, while the factors which altered the supply of terrigenous materials, as revealed by Dr. Rastall's work, did not produce any new conditions sufficient to induce an alteration in the life-assemblage, and there is nothing to suggest faunal changes connected with the passage of time, as apart from changed conditions of deposition. We conclude that these two beds belong to the same relatively short geological time-period and that the physical break between them, upon which Dr. Rastall lays so much stress, can mark at the most only a minor interruption in sedimentation, perhaps not amounting to an actual cessation.

In view of these similarities between the uppermost red bed and the Sponge Bed, we have always regarded the thin inconstant red seam separating them as something quite negligible when considering the question of continuity and conformity in the series. The irregularities in the form of the basal surface of the Sponge Bed give an added, and perhaps deceptive appearance of interruption at this level in the series. Such an irregular solution-surface at the base of a calcareous bed overlying a more impervious argillaceous layer is seen not infrequently in the case of the marl-seams which occur at many levels in the Chalk exposed both in coastal and inland sections. These argillaceous seams in some cases mark a temporary change in the character of the sedimentation. Sometimes they show an abrupt contrast to the more purely calcareous rock below and above them, and there may be the appearance of a break in sedimentation. Yet it is generally found that the assemblage of organisms is identical below and above the argillaceous band, even when this is of substantial thickness. Examples of this are to be seen on the Devon coast and in other districts. While the marl-seams are at times so thick, and so clearly interstratified with the more purely calcareous Chalk, as to point to an influx of argillaceous material, in other cases it appears not unlikely that the clayey constituent of the marl is a solution-residue, as suggested by Professor P. F. Kendall. But it may be said that, as a general rule, the intercalation of a marly or clayey seam in a Chalk succession cannot be taken, without strong supporting evidence, to point to a break in the continuity of a series. If Dr. Rastall's view as to the origin of the thin red seam is correct, it is clear that only a very slight break is indicated ; but we are far from being satisfied that his suggested explanation is the true one.

As regards the change in the assemblage of heavy minerals found in the red seam, and the absence of all these minerals from the Sponge Bed, it is our opinion that these facts, if accepted as fully established by Dr. Rastall's examination, may be of little or no significance in the question of continuity of succession. A principal factor in determining the amount and character of the terrigenous materials in any stratum in a given area is the nature of the transporting currents, particularly their direction and strength. Variations in these, as well as other causes such as shallowing or deepening of the sea-floor, or rising or falling of the land-surface, even in small degree, might produce marked local changes in sedimentation apart from any break in the succession of sediments. So far as minute and scattered heavy mineral grains are concerned, other factors might be enumerated; for in certain cases the distance of their transport from source of origin and their exact final distribution at a given time might depend on delicate local adjustments.

When we consider the complexity and wide range of the variable factors upon which sedimentation in any given area of the sea-floor depends, it seems to us that Dr. Rastall's deductions are put forward with insufficient reservation. He has indulged in some interesting though highly speculative suggestions regarding the sources of the minerals in the Carstone and the Red Rock, and of the red material which is disseminated through the Red Rock and, in more northerly districts, through a varying thickness of the Lower Chalk. Even if we consider those speculations to be much too bold, and built by ingenious scientific imagination upon an insufficient foundation, it is unnecessary here to enter into any special criticism of them. Our present theme is the question of the conformity and continuity of the Red Rock with the overlying Lower Chalk. It may suffice here to state that Dr. Rastall's work does not alter our opinion that the upper part of the Red Rock must be linked together intimately with the Lower Chalk, as thought by Judd, Barrois, Gunn, and Whitaker, If the line of junction really marks any pause in sedimentation, this, we think, can only have been of quite a minor character. The palaeontological evidence gives no indication even of a slight non-sequence, and we must regard that evidence of overruling importance. It is not without significance, also, that the Red Rock and the overlying Chalk, viewed in their broader field-relations, are seen to be distributed as a unit.

IV. Concluding Remarks.

In the foregoing discussion we have given reasons in support of our view that the Red Rock is unconformable with the Carstone upon which it rests, but has a conformable relation to the overlying Chalk. We have shown that the conflict in the results attained by Dr. Rastall and by ourselves arises entirely from the utilization of different evidence as the basis of interpretation. While he has founded his conclusions upon the apparent relations of the beds exposed in the Hunstanton and Snettisham sections, and upon facts ascertained by a detailed petrographical examination, he has ignored the definite indications given by the available palaeontological evidence and by a consideration of the broader stratigraphical relations of the Red Rock. Since the whole question at issue is the relation of the Red Rock at Hunstanton to the Carstone underlying it and to the Chalk above it, the use of the evidence most fitted for employment in geological time-determinations has seemed to us to be essential in a settlement. No excuse is needed for our preference to make use of palaeontological evidence, of a value for this purpose well established by long experience, in place of that provided by a comparison of mineral-assemblages which in themselves can have no weight in the question of time-determinations. We have also found it important, in examining the stratigraphical relations of the Red Rock, to draw conclusions from a wider range of facts than those ascertainable by considering only the succession seen at Hunstanton and Snettisham, so likely to mislead by its deceptive appearance.

The conflict appears, therefore, to result from the difference in the value (for the purpose in view) attached to these independent classes of evidence by Dr. Rastall and by ourselves. He had at command the whole of the facts which we have utilized in determining the relations of the Red Rock, and he supplemented these by a detailed petrographical investigation. Yet he chose to leave out of account all the evidence which we consider to be essential for ascertaining the true stratigraphical relations, and drew his conclusions from a different and, in our opinion, an insufficient set of data. We think that in adopting this procedure he was mistaken, and that his deductions, so far as they are contradicted by the palaeontological and stratigraphical evidence, must be unsound.

It is unnecessary to justify this opinion by entering here into any general discussion of the subject of stratigraphical correlation-work, and the utilization of the evidence most suitable for the attainment of sound results. The principles are thoroughly established and are known to all geologists. They have been amply developed and tested during the course of many decades; indeed, since William Smith's observations were made more than a century ago. Their daily application in practical work, such as geological mapping and the correlation of the strata passed through in borings or shaft-sinkings, provides continual confirmation. This touchstone of practical applications, made under specially exacting conditions when large commercial interests are involved, leads to a more and more critical and certain evaluation of the different categories of evidence called into service for these purposes. We therefore feel fully justified in saying that we have rightly selected the stratigraphical and palaeontological evidence as the essential and sufficient means for elucidating the relations of the Red Rock.

If this be conceded, it follows that our lack of the knowledge to be gained by an exhaustive petrographical study of the Red Rock and contiguous strata did not deprive us of evidence that was a necessary part of the basis for sound conclusions. Dr. Rastall, on the other hand, omitted the consideration of evidence that was essential, and confined his attention to facts which, while incapable of rebutting the indisputable data utilized by us, are in themselves an insufficient basis for the conclusions reached by him. We criticize him, therefore, for assailing our work by arguments drawn from a too restricted range of facts, and for allowing himself to exceed the legitimate use of the petrographical data upon which he too exclusively concentrated his attention.

REVIEWS.

THE VERTEBRATE FAUNAS OF THE ENGLISH EOCENE. Vol. i: From the Thanet Sands to the Basement Bed of the London Clay. By Dr. E. I. WHITE. pp. xiv + 123, with 162 text-figures. British Museum (Natural History). Printed by order of the Trustees. London, 1931.

THE chief purpose of this memoir is to describe the newly-discovered vertebrate fauna of the Blackheath and Oldhaven beds occurring in two localities, from both of which extensive collections have been made. The subject matter, however, is not limited to the new material, for all known records of vertebrate remains in the Lower Eocene are included with the exception of a few casual references to sharks' teeth of undetermined identity. After a brief account of the stratigraphy of the Lower Eocene, the actual records of specimens are given in the order of the deposits, the "new material" being described under separate headings. Tables of geographical distribution follow, and there is a short chapter on the "Analysis of the Faunas". The next and largest section of the memoir consists of a systematic description of the fishes, and is profusely illustrated. The work is concluded by two appendices—the first, by Mr. Allan Frost, on fish teeth, and the second, by Mr. Arthur Wrigley, on the Lower Eocene Mollusca of Abbey Wood and High Halstow, the two localities in which the recent discoveries of vertebrate remains were made. The work is a useful addition to the series of museum memoirs.

The section entitled "Analysis of the Faunas" contains some suggestive remarks on the value of the present distribution of surviving genera as a guide to the climatic conditions obtaining during former geological periods, a question discussed at some length at a recent meeting of the Royal Society. Thus, it is pointed out that the mean annual temperature of the sea in a particular area may be tropical or sub-tropical, while the climate of the adjacent land may have been neither. Moreover, with Palaeozoic or Mesozoic organisms of long descent it is reasonable to assume a considerable amount of adaptability to varying conditions. The author remarks further that adaptability may be a generic (or even a specific) character, and that instances of localized and cosmopolitan species belonging to the same genus are certainly known. Such considerations are often lost sight of by palaeontologists, and Dr. White has done well to give them prominence in this review of the English Eocene vertebrate fauna.

F. H. A. M.

COPE: MASTER NATURALIST. The Life and Letters of Edward Drinker Cope, with a Bibliography of his Writings classified by subject. By HENRY FAIRFIELD OSBORN, with the co-operation of HELEN ANN WARREN. pp. xvi + 740, illustrated. Princeton University Press (London: Humphrey Milford, Oxford University Press), 1931. Price 22*s.* 6*d.*

THE additional title of this book is " A Study of the Pioneer and Foundation Periods of Vertebrate Palaeontology in America ". It begins with an account of the opening up, one after another, of the celebrated fossil-bearing deposits which have supplied such abundant palaeontological evidence of the doctrine of descent among vertebrate animals. In those early days Joseph Leidy was the great pioneer and interpretative spirit and we are told how Cope, by attending Leidy's lectures, came under his strong scientific influence. The story of the long series of investigations which resulted, with some account of Cope's general conclusions as to the phylogeny of the vertebrate and other matters of scientific importance, is fully recorded. The bulk of the work accomplished is only partly indicated by the titles of the papers which Cope wrote; the record of these occupies nearly 150 pages in the bibliography, each page containing some ten or more references.

A great many pages are devoted to Cope's letters to members of his family and friends, and these reveal a pleasing and very human personality. The full details of the decidedly unpleasant newspaper controversy between Cope and his great rival O. C. Marsh might advantageously have been omitted, and the same may be said of some of the passages in his correspondence, as they tend only to belittle one who was undoubtedly a great man and a master naturalist. Born and bred a member of the Society of Friends, Cope's views broadened as his mind developed, but he never threw off his fundamental religious beliefs. Towards the end of the volume there is a delightful letter written by Cope to his Aunt Jane on the occasion of her birthday, and this reflects his mature religious philosophy. The volume as a whole is of great interest, both as a history of palaeontological discovery and as a record of the life and work of a distinguished man of science.

F. H. A. M.

Discussion on the Validity of the Permian System. British Association, Bristol, 1930 (ordered by the General Committee to be printed in extended form). Report. pp. 315–25. B. A. Reprints, New Series, No. 28.

IN the opening paragraph of this publication Dr. Bernard Smith suggests that the controversy as to the validity of the Permian system is likely to cause even more expenditure of energy than that attaching to the limits of the Cambrian and Silurian in the days of Sedgwick and Murchison: the reviewer, not having been present, is unaware whether "chunks of *New* Red Sandstone" played any material part in the actual discussion.

Several speakers wisely and pertinently laid stress on the futility of trying to arrive at any general conclusions as to correlation from the uppermost Carboniferous, Permian, and Triassic deposits of Britain, or even of Northern Europe, which are of the continental facies. Such deposits must of necessity be full of discontinuities, since the land is an area of denudation, not of deposition. Correlations in areas of "red" facies must of necessity depend mainly on lithology, which is always an unsafe guide: lithological facies depend on physical conditions, which in turn depend on earth movements. Therefore if at any given time an earth-wave is travelling forwards in any direction similar conditions depending on it will prevail in different areas at different times. The doctrine of homotaxis applies to lithology as well as to palaeontology.

It is evident that the question of the Permian must be fought out on a wider field than Europe affords. Much prominence has been given to the views of Professor Schuchert, who claims enormous thickness for Permian strata. This is partly based on his assignment of the Gondwana tillites to a horizon in the Middle Permian, but with this few geologists acquainted with the southern hemisphere will be found to agree. The subject cannot here be discussed in detail, but the general consensus of opinion certainly suggests a location well within the Upper Carboniferous as defined in Europe and America. In the Worcester district of Cape Colony there is no visible break between the base of the Cape system (Table Mountain Sandstone) and the Ecca Series; in view of the well-established Devonian age of the Bokkeveld fauna, one cannot but wonder what Professor Schuchert supposes to have happened to the Carboniferous if the Dwyka is Middle Permian. The Witteberg Series seems an inadequate representative of the Carboniferous and Lower Permian. Perhaps, however, these are represented by the wholly gratuitous "discontinuities" and "great hiatus" introduced into his correlation table in the *Symposium on Continental Drift*, which have no real existence. It is true that there is a big overlap at the base of the Dwyka, but this is to the *north*, not to the south, on to the Cape Mountains, as stated by Professor Schuchert.

The next point is as to marine equivalents of the Permian. In the discussion much stress is laid, and rightly laid, on the occurrence of rich Permian marine faunas in Texas and Timor, among others. Let us consider for a moment what the announcement of a discovery of " the richest marine Permian fauna in the world " really means. Such an announcement, when analysed, really amounts to this: a recently discovered fauna with many new species contains also forms which somebody has called Permian in some other locality: it is just arguing in a circle, and as the definition of a Permian fauna started off with queer abnormal assemblages in North-West Europe, it can only be called a vicious circle.

A good account of the Permian fauna of Timor and neighbouring islands will be found in Brouwer's *Geology of the Netherlands East Indies.* In this connection it may be mentioned that in the northern part of the Malay Peninsula there is every reason to believe that the great marine limestone of the Raub series (a typical development of the Carboniferous Limestone facies) passes up with little or no break into the Trias, which is also marine of a sort, and in which Rhaetic fossils have been described on good authority. There appears to be no need at all for a Permian system here, and it is not very far from Timor. The contrast between this area and Gondwanaland is violent, and yet it lies directly between India and Australia. Its affinity, if any can be traced, is with the Himalayan region.

In view of the peculiar characters of the Carboniferous, Permian, and Trias in Northern Europe, where these systems were first defined, and in North America, the question may legitimately be stated in this way, so far as regards the marine strata: the Goniatite faunas of the Carboniferous, so far as they go, are quite definite; the Ammonite faunas of the Southern Trias (Tethys) so far as they go, are quite definite: somewhere between them is a transition, if there is anything in evolution. Where is this transition, and if it exists, are any part of the strata containing it sufficiently well-defined to constitute a system apart from the Carboniferous and Trias? This, it seems to the writer, is the problem of the Permian.

CORRESPONDENCE.

AN UNKNOWN KELLAWAYS LOCALITY IN DORSET?

Sir,—Some palaeontological evidence bearing on off-shore tectonics and the underground structure of the Chesil Beach was observed recently when examining with Mr. L. Richardson the banks of the West Fleet Backwater. One mile west-south-west of Langton Herring Church where the Fullers Earth oyster-bed crops out in the northern limb of the Weymouth anticline and the shore is formed of Fullers Earth Clay, large numbers of Callovian fossils were found washed up along a stretch of about 100 yards.

The commonest species are *Trigonia irregularis* Seebach, *T. elongata* Sow., *Liostrea undosa* (Phil.) and *Lopha marshii* (Sow.), with numerous fragments of flattened *Proplanulites*, some having *Serpulae* attached.

Many of the *Trigoniae* are broken, but large numbers, although very fragile, are whole and unworn, and may be collected in an exceptionally perfect state of preservation. The nearest point at which Callovian or higher beds touch the shore is distant 2 miles in one direction and 2½ miles in the other. It is inconceivable that any of the fossils can have travelled 2 miles along the coast without becoming broken or worn. Moreover, on their journey they would have to pass localities where the shore is covered with Cornbrash fossils, and others where it is almost made of Bradfordian brachiopods from the *Rhynchonella boueti* Bed, yet they have collected none of these on their way, nor have they left any trail along the intervening beaches. Long-shore drift in the Fleet is, in fact, so slight that the outcropping fossil-beds contaminate only small areas.

The inference seems justifiable, even unavoidable, that opposite this point on the coast there is a submerged tract of Kellaways Beds faulted down against the middle of the Fullers Earth. As few of the pebbles from the Chesil Beach cross the Fleet to the opposite shore, it is likely that the shells are being weathered out of the bed of the backwater.

Incidentally, the locality provides a striking illustration of how mistakes may have crept into old records. These shells have obviously come from a clay matrix and they lie upon a clay foreshore, in which other oysters may be seen *in situ*. What more natural than to regard them all as belonging to the Fullers Earth ?

W. J. Arkell.

14 Chadlington Road,
Oxford.

THE CLAY PEBBLE BED OF ANCON, ECUADOR.

Sir,—Dr. Busk's letter in your May issue having reopened the discussion concerning the origin of the Clay Pebble Bed of Ancon, Ecuador, I should like to state that after ten years' study of the tectonics of north-west Peru I have proved beyond doubt that the Tertiary Rocks from Payta in Peru to the Santa Elena Peninsula in Ecuador have slipped down the steep slope on which they were deposited. This conclusion is based on several hundred square miles of large scale geological mapping and the examination of 800 well logs representing 1,700,000 feet of drilling.

The result is, as Dr. Busk states, a gigantic tectonic breccia, at least 200 miles long and 20 miles wide. Clay Pebble Beds are common throughout, though that at Ancon is the best example.

The movement has taken place along huge *slip planes*, which are in every way similar to thrust planes except that they are normal faults of low angle instead of reversed faults, so that instead of repetition of strata there is elimination of several thousand feet. The lateral shift of 5 km. mentioned by Dr. Busk is of similar magnitude to numerous examples I have studied, and the total slip of the uppermost strata over the lowest must be at least 10 miles.

The "thrust plane" of our original paper is undoubtedly one of these slip planes, which I hope to describe in detail when I return to England next year.

R. A. Baldry.

Lobitos,
Talara,
Peru.
2nd August, 1931.

COAL MEASURE FLORAS.

Sir,—I should like to be allowed to draw attention to the important conclusions reached by Dr. R. Crookall in his recent "Critical Revision of Kidston's Coal Measure Floras" (*Proc. Roy. Phys. Soc.*, xxii, 1931, p. 1). The supposed absence of any equivalent of the Lanarkian Series in South Wales, maintained on palaeobotanical evidence, has been disputed by workers on both marine and non-marine faunas, and it is gratifying to find that Dr. Crookall now recognizes that its supposed absence was due to inadequate collecting, Dr. Crookall also shows that a portion of Kidston's Westphalian (now known as Yorkian) in Yorkshire is equivalent to a portion of his Lanarkian in Scotland. Similar conclusions have been reached independently by Miss E. Dix, and it therefore appears that there is no serious discrepancy in the correlations based on faunas and floras (cf. Dix, Pringle, and Trueman, *The Naturalist*, 1930, p. 324).

Dr. Crookall proposes for the present to retain Kidston's term Lanarkian and to include in it, broadly, the Lower Coal Measures and Millstone Grit of Yorkshire and their equivalents. He would place the top of the Lanarkian in Stirlingshire at approximately the level of the Auchingane Coal, whereas Kidston in 1916 selected the Ell Coal; this change involves transferring to the Yorkian about three-quarters of those Scottish Coal Measures which Kidston placed in his Lanarkian, and at the same time transferring to the Yorkian a great number of the records which constituted Kidston's Lanarkian flora. It is apparent that Kidston's term Lanarkian was ill-defined, both palaeontologically and stratigraphically, and has already led to much confusion; I hope Dr. Crookall will agree to a revised terminology when the sequence is sufficiently known.

A. E. Trueman.

University College,
Swansea.
4th October, 1931.

[As the above letter suggests a revision of Coal-measure terminology, the Editor, as a Yorkshireman, ventures to express his earnest hope that that horrible word "Yorkian" may disappear from the literature. From the county aspect, owing to the very large number of geological formations there occurring, it is pointless, while the city of York does not stand on Coal-measures.]

PEGMATITES, APLITES AND TIN VEINS.

SIR,—I was much interested in Dr. Derry's article on the "Genetic Relationships of the Pegmatites, Aplites and Tin Veins" in the October's number of the Magazine.

My own observations on such veins in Cornwall tend to support Dr. Derry's conclusions, but there are mineral associations in this area which he does not mention and some differences of vein relations which may be worth noting.[1]

In the first place Cornish pegmatites contain molybdenite, arsenopyrite and wolframite in addition to cassiterite: they are also rich in lithia-mica, fluor-mica and fluor-apatite, while the aplites are often rich in topaz. Fluorite is a frequent constituent in small proportions and occasionally in large amount.

There are also instances of mineral veins with pegmatite-like structure composed of large crystals of wolframite with arsenopyrite, stannite, quartz, and interstitial pink aplitic material.

Again many of the Cornish pegmatite veins show banded structure resulting from alternate layers of aplite and pegmatite. This type does not seem to have been observed by Dr. Derry.

The Manitoba pegmatites are a most interesting series of veins and I think that Dr. Derry's conclusion that the existence of stanniferous pegmatites is no reliable indication of the existence of tin veins in the same area is quite justified.

E. H. DAVISON.

44 MOUNT PLEASANT ROAD,
CAMBORNE,
CORNWALL.
12th October, 1931.

ANNOUNCEMENTS AND INQUIRIES.

GEOLOGICAL CONFERENCE AT KIGOMA, TANGANYIKA TERRITORY.

FROM 7th to 20th July, representatives of the Geological Surveys of Southern Equatorial Africa met at Kigoma to discuss the compilation of a geological map of this part of the Continent. The Conference constituted the first meeting of the Sub-Commission of African Geological Surveys formed at the XVth International Geological Congress at Pretoria in 1929, and the countries represented at Kigoma were as follows: French Equatorial Africa, Northern

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[1] *Handbook of Cornish Geol.*, pp. 46, 47. *Rep. Tin and Tungsten Research Board*, 1922. *Rep. Brit. Assn.*, 1927.

Rhodesia, Belgian Congo, Ruanda Urundi, Uganda, Tanganyika Territory, and Nyasaland.

In the various countries named provisional geological maps already exist, but great difficulty has hitherto been found in establishing the relation between the geological formations of any one country with those of neighbouring or more distant countries, and this difficulty has been greatest in the case of the ancient unfossiliferous formations. In the course of the recent discussions, however, assisted by a comparison of rock specimens and maps from the different countries, considerable progress has been made, and as a result of this the Sub-Commission has drafted a geological map of Southern Equatorial Africa which it is proposed to publish at an early date. A great advance has been made in the mapping of the Katanga System, within which lie the great copper deposits of Northern Rhodesia and the Belgian Congo, and the tin-bearing formations of Uganda can be followed through Tanganyika and Ruanda Urundi to the Belgian Congo. Moreover, the Karroo System, bearing many coal deposits in this part of Africa, was discussed in some detail.

Since the only sound basis for the development of mineral resources is a thorough knowledge of the geological formations and of their relations to one another, the discussions at the Conference and the conclusions arrived at, as summarized in the new geological map, should prove to be of considerable economic value as well as of scientific interest.

The Conference recognized the following three important systems of pre-Karroo age :—

(*a*) The Basement Complex, comprising sedimentary and igneous groups showing wide variations in degree of metamorphism ; this system is roughly comparable with the Swaziland System of South Africa.

(*b*) The Muva-Ankole System, consisting principally of quartzite and shales, with acid volcanic rocks locally, and, more rarely, basic rocks. In some areas the shales are represented by phyllites or schists, and the quartzites show a similar range of metamorphism. The System can be traced from Northern Rhodesia to Uganda, and includes, for example, the Muva, Mafingi, Ukinga, and Karagwe-Ankolean groups. It is separated from the Systems below and above by vast unconformities, and it is considered as probably equivalent in part to the Witwatersrand and the Ventersdorp Systems.

(*c*) The Katanga System, ranging from the Série des Mines to the Upper Kundelungu, and equivalent to the Transvaal-Nama and Waterberg Systems. At the base of the Lower Kundelungu is the great tillite of the Katanga, so that in this area at least one important glacial epoch of pre-Karroo age is recognized. It may be added that over a wide area in this part of Africa glacial deposits are believed to exist at the base of the Karroo, although their correlation with the Dwyka has not yet been established.